THE

AËRONAUTICAL JOURNAL

(FOUNDED 1897 in succession to the ANNUAL REPORTS)

The Organ of the Aëronautical Society of Great Britain

Telephone:
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"DIDASKALOS, LONDON."

EDITED FOR THE COUNCIL BY
T. O'B. HUBBARD.

Published Quarterly

Subscription per Annum, post free, 6s. 6d.

All Communications should be addressed to—
THE EDITOR, 11, Adam Street, Adelphi, W.C.

No. 63. JULY, 1912. VOL. XVI.

Notices of the Aëronautical Society of Great Britain

GENERAL NOTICES

May Elections: *Members:*—ALEXANDER GROSS; MAJOR K. J. KINCAIRD-SMITH. *Student:*—F. E. NANCARROW.

July Election: *Life Member:*—T. O'B. HUBBARD.

¶ **Secretaryship.** Mr. BERTRAM G. COOPER has been appointed Secretary of the Society from August 14 in place of Mr. T. O'B. HUBBARD, who is entering the Special Reserve of the Royal Flying Corps. Mr. COOPER, in accordance with the Rules, has resigned from the Council in order to take up this appointment.

¶ **Associate Fellowship Election.** As the result of the recent election, the following have been elected Associate Fellows:—

DR. F. A. BARTON
A. E. BERRIMAN
R. BLACKBURN
HARRIS BOOTH
F. H. BRAMWELL
MAJOR H. R. M. BROOKE-POPHAM
MAJOR C. J. BURKE
COL. J. E. CAPPER, C.B., R.E.
PROF. HERBERT CHATLEY
B. G. COOPER
HORACE DARWIN, F.R.S.
H. FERGUSON
EDWARD P. FROST
COL. J. D. FULLERTON
B. MELVILL JONES
H. F. LLOYD
MAJOR E. M. MAITLAND
F. K. MCCLEAN
G. F. MORT
A. P. THURSTON
W. R. TURNBULL
HOWARD T. WRIGHT
W. ELLIS WILLIAMS

¶ **Associate Fellowship Examination.** An examination of Candidates for admission to the Associate Fellowship will be held on Wednesday and Thursday, March 12 and 13, 1913. Rules and syllabus will be circulated in due course.

¶ **Associate Fellows Meeting.** A Meeting of Associate Fellows will be held at the offices of the Society on Wednesday, September 4, at 5.30 p.m.

¶ **Wilbur Wright Memorial.** A Subscription List has been opened to found a Memorial to our distinguished member, the late Mr. Wilbur Wright, as an appreciation of his great work and also as some small recognition of the support he gave to this Society. The Memorial is to take the form of a Premium Lecture on Aeronautics to be delivered annually, and to be called the "Wilbur Wright Lecture."

The fund will be administered by trustees, and the following have consented to act in this capacity:—Lord Northcliffe, Griffith Brewer, Alec Ogilvie, Col. H. E. Rawson, C.B., B. Woodward, and the Chairman of the Council (*ex officio*).

Cheques should be forwarded to the Secretary, made payable to "The Wilbur Wright Memorial Fund" and crossed "Messrs. Coutts & Co."

The following subscriptions have already been received:—

	£	s.	d.
P. Y. Alexander	100	0	0
Lord Northcliffe	50	0	0
Alec Ogilvie	50	0	0
Messrs. Short Bros.	50	0	0
A. Mortimer Singer	50	0	0
F. K. McClean	26	5	0
Griffith Brewer	21	0	0
Hon. Maurice Egerton	20	0	0
Estate of the late Cecil S. Grace	10	10	0
Messrs. Handley Page, Ltd.	10	10	0
Mervyn O'Gorman	10	10	0
Royal Aero Club	10	10	0
T. P. Searight	10	0	0
R. Runge	10	0	0
M. Atkinson Adam	5	5	0
Messrs. George Barker & Brettell	5	5	0
Rt. Hon. A. J. Balfour, M.P.	5	0	0
Messrs. Abel & Imray	3	3	0
W. M. Jackson	3	3	0
J. T. C. Moore-Brabazon	3	3	0
Major-General R. M. Ruck	3	3	0
Col. J. E. Capper, C.B.	2	2	0
V. Le Cren	2	2	0
Lieut. J. R. F. Lecky	2	2	0
G. F. Mort	2	2	0
Capt. H. F. Wood	2	2	0
F. S. Lahm	2	0	0
Major B. F. S. Baden-Powell	1	1	0
A. E. Berriman	1	1	0
Harris Booth	1	1	0
T. W. K. Clark	1	1	0
Dugald Clerk	1	1	0
Carried forward	475	2	0

	£	s.	d.
Brought forward	475	2	0
Horace Darwin	1	1	0
J. W. Dunne	1	1	0
Miss M. Dunne	1	1	0
G. G. M. Hardingham	1	1	0
B. E. D. Kilburn	1	1	0
J. G. Lorrain..	1	1	0
Capt. E. M. Maitland	1	1	0
Sir Hiram Maxim	1	1	0
Col. H. E. Rawson, C.B.	1	1	0
T. W. Rogers	1	1	0
Sir Chas. Seely, Bart.	1	1	0
W. R. Turnbull	1	1	0
Col. H. E. Tyler	1	1	0
Mrs. Wenham	1	1	0
H. Massac Buist	1	0	0
W. H. Dines	1	0	0
Hon. Henry S. Littleton	1	0	0
B. G. Cooper	0	10	6
H. D. Cutler	0	10	6
Capt. Nevill Eliot	0	10	6
F. M. Green	0	10	6
T. O'B. Hubbard	0	10	6
H. F. Lloyd	0	10	6
John MacHaffie	0	10	6
Col. H. S. Massy	0	10	6
Alex. McCallum	0	10	6
A. P. Thurston	0	10	6
C. C. Turner	0	10	6
H. A. Turrill	0	10	6
Capt. W. J. P. Rodd	0	10	0
J. Green	0	5	0
H. F. Jackson	0	3	0
Total	£500	0	0

¶ **The late Mr. P. Williams.** We regret to announce the death, after a long illness, of Mr. Peter Williams of Ruabon, on June 24 last. Mr. Williams had been a member of the Society since 1902.

¶ **Gift to the Library.** The Council desires to thank Mr. T. O'B. Hubbard for the gift of a collection of 120 books and pamphlets on aeronautics.

WILBUR WRIGHT

Gold Medallist of the Society

BY GRIFFITH BREWER, A.F.AE.S.

THE present generation will never completely realise the magnitude of the loss to Aviation caused by the death of Wilbur Wright, and yet it requires but a moment's thought to recognise that his name will live until all histories are obliterated, for with his brother Orville, he accomplished the dream of ages, which men of acknowledged science had become in the habit of first attempting to accomplish, and then alleging the feat to be amongst the unattainable.

There are only three ways of getting from one place to another on this earth; one is on land, the next is on water, and the third is now through the air. Certainly it was possible to travel in the air to destinations settled by the caprice of the wind, but to do the same as the birds, and travel from one place to another, had never been possible until Wilbur and Orville Wright accomplished mechanical flight in 1903. So great was this achievement, that when Wilbur Wright came to France to show how this thing could be done, the doubters came from all over the world to see with their own eyes the achievement of the impossible.

In August, 1908, I went to Le Mans to see with my own eyes what I, like others, had hitherto doubted, and when the flying was over for the day and the machine had been put back into its shed, Mr. Wright invited me to dine with him at the little Inn, where Madam Pollet served a plain, well-cooked meal in the little sanded kitchen.

I found Wilbur Wright quite open about his work; there was no air of mystery or cloaking the achievement of flight with artificial embellishments. Just the simple story of two brothers who had always played together and worked together, and one whose health required constant out-door exercise. At first they made a glider, which not being built upon books, had none of the difficulties enumerated in them, but had others not previously hinted at; and he told how in their holidays which grew longer as their work time grew less, they gained more experience in the art of gliding until they decided on the details of the power-driven machine. And as he told me of the fun they had had at Kitty Hawk with the hot wind blowing on the little tin shed and the thrills enjoyed in the first flights, I realised what lucky brothers they were, to have been blessed with the necessary intelligence, perseverance and love of roughing it, to make such glorious holidays brimful of recreation and enjoyment and yet so useful to mankind. Few know how the experiments were nearly abandoned on some occasions, and how their rival flyers the mosquitoes combined together in their millions to prevent the invasion of their element by man. How many resolutions to pack up and go home to-morrow were made in the night when those tiny insects, which in their myriads blackened the walls of the shed, succeeded in entering the blankets in which the brothers rolled themselves in spite of a night temperature of 90°. Fortunately the tortures of the night were forgotten in the enthusiasm of the day, and so they persevered and robbed the birds of their monopoly.

Many of my early meetings with Wilbur Wright were associated with the late Hon. C. S. Rolls. It was in August, 1908, when Rolls found other engagements prevented his taking part in the Gordon Bennett Balloon Race from Berlin, that he asked me to dine with him and talk over the details of my racing in his stead. He had just returned from Paris, and I was leaving for Paris that same evening, and after we had talked balloons and arranged for Mr. McClean to act as my aid in the race, he turned to me and said, "Why are you going to Paris?" In reply, I said: "Don't tell anyone, but I'm going on to Le Mans to see Wilbur Wright." "Well, don't say anything," Rolls replied, "but I've just come back from there." He then gave me some local directions about how to get to the Champ d'Auvours and where to stay at Le Mans. Afterwards I always stayed at the little Inn near the railway, because then I could be out early if the weather were fine, and in this way I saw many flights I should otherwise have missed.

The simple life had great attractions for Wilbur Wright. A piece of stout canvas nailed between two pieces of 2 in. by 3 in. wood supported at their ends on the rafters of the shed containing the machine, formed the bed on which he slept, and often at five

Born April 16, 1867—Died May 30, 1912

in the sparkling September mornings he was to be seen taking an early cold water wash while the water for the coffee was on the boil. The machine was always his first care, for it was the only machine available and it served for all the flights of those last four eventful months of the year 1908.

The same shed, which by the by was too small to take the machine until the rudder had been folded back, served as his office as well as his bedroom, and as may be well imagined, the popular interest in those early flights attracted a large and in some cases unwelcome correspondence. He had no secretary to look after his letters, and most of his time being taken up with the machine, flying, and answering questions, correspondence could only have very limited attention. The most important letters were attended to at the time, and the remainder were placed in the pile to be answered when time permitted. Many persons who wrote did not seem to realise that Wilbur was one of the hardest worked men, who could not spare time to gratify the idle curiosity of the thousands who were naturally interested.

Some weeks later, just before the Gordon Bennett Race in Berlin, I came again to Le Mans in the hope of having a flight. Mr. Rolls, Mr. Butler and Major Baden-Powell were also there, all with the same hope in their minds. We were so infected with the flying fever, that we were tumbling over each other in the hope of getting the first invitation, and this hope was perhaps intensified by the knowledge that up to that time no Englishman had yet flown. Poor Rolls was torn by two anxieties, one to be given the first flight, and the other to get me off to Berlin so that I should not be too late to compete as his substitute in the Gordon Bennett Race, and the latter fear predominated to such an extent, that he most unselfishly suggested to Wilbur that I should go up first. Whether this turned the balance in my favour or not I cannot say, but after making other trial flights Wilbur came to me and asked if I was ready, so I took my place between him and the engine, and we were launched off the rail and made a three mile flight round the ground. It is somewhat late now to describe the sensation of flying which so many thousands have since experienced, but a first flight like this must be unique. I remember wondering whether we should really rise from the rail, and then a feeling of elation when the grass slipped away backwards and downwards and the machine seemed to be sitting on nothing. There was no sense of travelling except by the appearance of the earth moving backwards, and on looking upwards it seemed that we were on a frame structure in a high wind, but without the sense of movement inherent in all other vehicles which are supported below on wheels or on water. In spite of knowing the theory of banking on a turn, I remember marvelling at the machine not slipping inwards when inclined to such an angle. I am afraid, however, that my keenness of observation of the flight sensations was stifled by my greater interest in the man than in the doings of the machine, and the predominant sense was one of wonder that the same man could calmly invent such a mechanism and yet fly it with such consummate skill. Mr. Rolls made his first flight immediately afterwards, then Mr. Butler, and later in the day Major Baden-Powell.

In November of the same year I introduced a party of Englishmen to Wilbur Wright, viz., Mr. Roger Wallace, K.C., Lord Royston, Professor Huntington, Mr. McClean, and Mr. Eustace Short. Having accidentally missed Wilbur in Le Mans, we returned to Paris, where we met him next day, and he entertained us to lunch. Afterwards Mr. Wallace, Professor Huntington, Mr. McClean and Mr. Short returned to Le Mans and were given short flights the following morning, as a compensation for their fruitless journey to Le Mans the previous day. Wilbur never took a fee for any of these passenger flights, and when it is remembered that many people were willing to pay hundreds of pounds for a flight in those early days, these four special flights were a favour of no small character.

During the last three months of the year 1908, when Wilbur beat all his own records and set up new world's records for duration, distance, and altitude, scientists and notabilities came to Le Mans from all parts of the world, many of the former affirming that they had always believed in the flights made by the Wrights in America in 1903 to 1905. Others made numerous suggestions of how to simplify what they regarded as a clumsy apparatus. One distinguished scientist, after looking at the machine for some time, asked Wilbur if he would allow him to measure the camber of the planes, because this would enable him to estimate the

length of the undulations followed by the machine in flight. Having been given permission he made a complicated series of calculations and then asked if the length he mentioned was the length of the waves of flight. In reply, Wilbur said that the length of wave was not correct, because the machine flew straight, and not on an undulating path. The scientist was astonished, and replied, " Impossible," to which Wilbur, ever a pioneer in all he did, replied, " Wait and see." Surely enough the scientist did see the machine make a straight flight soon afterwards, and it is to be hoped that by this time he has forgiven it refusing to follow the laws of the air, which were regarded as infallible at that time.

In January, 1909, Wilbur Wright went to Pau to continue his experiments and to teach the first three pupils how to fly. I was prevented from going by having to undergo an operation, but as soon as I was convalescent early in February, Mr. Rolls gave me a seat in his car and with Mr. Massac Buist we went leisurely across France to Pau, where by that time Wilbur had been joined by his brother Orville and his sister, Miss Katherine Wright. I think it was at this time that I more fully realised how kind and thoughtful these three Americans were, for although kings and world-known men were continually there to see the flights, one or other of those three always found some time to stay by me as I lay on my rug on the flying ground. This was the first opportunity I had had of hearing the two brothers discuss the machine and its doings, and it soon became obvious how suited they were to thrash out the many intricate problems which continually arose. Nothing was ever taken for granted until proved by themselves in actual practice ; all the hitherto recognised rules of wind and air currents were forgotten and replaced by new tables in writing contained in little pocket books ; and in the arguments, if one brother took one view, the other brother took the opposite view as a matter of course, and the subject was thrashed to pieces until a mutually acceptable result remained. I have often been asked since these pioneer days, " Tell me, Brewer, who was really the originator of those two ? " In reply, I used first to say, " I think it was mostly Wilbur," and later, when I came to know Orville better, I said " The thing could not have been done without Orville." Now when asked, I find I have to say, " I don't know," and I feel the more I think of it that it was only the wonderful combination of these two brothers who devoted their lives together for this common object, that made the discovery of the art of flying possible.

Early in 1909 an American millionaire, with a confidence born of unlimited dollars, came to Pau intending to make a flight with Wilbur, and talking to one of the " English Bunch," he said he had come for that purpose. The Englishman expressed surprise, because as he said he understood Wilbur was not taking passengers, to which the American replied, " Oh ! I daresay that can be arranged." " Well," said the Englishman, " I should like to be around when you do the arranging, just to see how it's done." What reply he got to his suggestion will never be known, but he left Pau without having had a ride in the air.

One day we were working together in the little office at Pau, when a card was brought in bearing the name of a lady of title. " That lady, who is an entire stranger," said Wilbur, " wrote saying she was coming here next week, and so as she had settled to come, there was no need for me to reply. Then she telegraphed yesterday to say she would come to-day, and as she had also settled this definitely, there was no necessity for me to reply to this wire. Now she is out in the ground and wants to waste my time without any kind of excuse." With that, the card joined the rest of the correspondence in suspense, and we resumed our work. Later, Wilbur went out and made a pretty flight, which was no doubt viewed by the lady along with the other spectators. I wondered at the time whether it would have been possible for him to gratify everyone, but it was evident after consideration of the circumstances that it would not have been feasible to do so. I also wondered whether the lady herself would be guilty of leaving a letter addressed to her unanswered. I have since been able to satisfy this last speculation, by recently writing to ask her for a small subscription to the Wilbur Wright Memorial Fund, to which letter I have received no reply.

When in Paris, just before the visit of Wilbur and Orville Wright to England, when they received the gold medals of the Aero Club and the Aeronautical Society, and were fêted by both those bodies, I was describing some of the people to Wilbur whom he would meet in England. Of one I said that he would at once recognise him as being the ugliest

man at the Aeronautical Society, and my rudeness was quietly reproved by his replying that the member of the Society would lose that distinction on this occasion because, as he said, "There will then be a pair of us."

Discussing happiness one day, Wilbur remarked that most enjoyment in life consisted of relief from discomfort. To try to be always comfortable and happy was therefore a mistake, for if one succeeded life became unbearably monotonous.

Discussing cricket as compared with baseball, Wilbur described most vividly the excited enthusiasm raised by the American game. His own words in a letter to me of the 9th July last year, from Berlin, make an interesting comparison between German cricket and American baseball:—

> "I would I could go with you—but it seems that it is impossible. A pleasant voyage to you.
>
> "To-day I took a stroll down to Tempelhofen field, where Orville did his flying in Berlin. I was rather surprised to find three or four games of cricket going on in different parts of the field. You will wonder what cricket looks like and sounds like in Dutch, but as I have never really seen a game in England, I do not know how to compare it with the genuine article. While you are in America get Orville to take you to see a baseball game. The conduct of the crowd will interest you immensely whether the game does or not. A baseball game at a school for deaf mutes would be noisy compared with this German crowd at a cricket game."

Ever since I have enjoyed Wilbur's friendship, I have found difficulty in writing the articles on aeronautical and atmospheric subjects that I have been in the habit of doing occasionally for some twenty years past. The reason for this is not that he has in any way abstracted the little knowledge I formerly possessed, but his lucid arguments and unanswerable information gradually altered most of my views, and whenever I have since tried to work out some problem or theory independently, I have found myself adopting and arguing some theory of Wilbur's, forgotten for the moment, but recognised when committed to paper. At first I used to mention Wilbur's name in acknowledgment, but after a time I found I had to mention him always, so I ceased to give him credit. This latter course seemed preferable as involving myself alone, and my confession now that I owe everything to him may atone for these omissions.

Describing an adventure by Parmelee on the Mexican border during the last civil war, Wilbur wrote in April, 1911, saying:—

"They were flying over a country where for a hundred miles there was no landing-place except the Rio Grande River. The cut-off cord got wet and shrunk so that it pulled the cut-off handle till one cylinder stopped exploding, so they gradually began to sink towards the river. Parmelee tried to make Lieut. Foulois understand what the trouble was, but could not succeed until too late. Finally just as they were touching the water Foulois "caught on" and pushed the lever back, but the effect was to turn the machine downward a little and make it dive for the bottom of the river. The next thing Parmelee knew he was sitting on the rocks at the bottom of the river with eighteen inches of water over his head. He lost no time in seeking higher atmosphere. Foulois' head soon appeared above water also, but as the lieutenant is only 5 ft. 1 in. tall, and the water was over 4 ft. deep., there was a margin of only about an inch under his chin. The banks were so steep that they had to wade down stream a quarter of a mile before going ashore. The machine was not much injured, and is all right again.

"It was a foolish trip undertaken without proper precautions, and while the cause of the accident was ridiculously trifling, the results might have been very serious."

Wilbur Wright was never an advocate of excessive speed, and in a letter of extraordinary technical interest to the pilot of the Wright machine in the Gordon Bennett of 1911, he said:—

"I had seen from the newspapers that you had finished the course and escaped without any smash-up. These were the only things I really cared about in the race. A matter of a few miles speed more or less was not a serious matter. Your troubles would be amusing if they were not so heart-breaking while they were going on.

"You are quite right in thinking that at high speeds the head resistances become very important, but I see that you are fooled on several points. If you stop to figure up you will find that 80 miles an hour is the speed a falling body attains after a fall of 256 feet. While 40 miles an hour is attained in less than 60 feet. So that a Nieuport cut off at 80 miles and landing at 40 has used up the equivalent of 200 ft. in addition to its actual drop. It ought therefore to go $6 \times 230 = 1380$ ft. before landing, if cut off at 30 ft. height. Your speed the day your motor struck with you was probably not more than 45 miles so you had no surplus energy. A machine with two propellers naturally does not glide as far as with a single propeller.

"Comte de Lambert tells me that the Nieuport motor is 135×145 and runs 1400. Now $\frac{135^2 \times 2 \times 145}{111^2 \times 4 \times 101}$ is in the proportion $\frac{528}{497}$ so that it is a sixteenth larger than our 4-cylinder motor. The speed of the Nieuport with 100 h.p. motor was about what Brookins had with not over 70 h.p. last year. We built that machine to prove that the talk about the speed advantages of the monoplane and the fore and aft arrangements was largely rot. It was faster than any of the 100 h.p. monoplanes of its time.

"I have written a letter to the *New York Herald* proposing that in 1912 no motor having a total piston area of more than 665 sq. centimetres and 8,000 cubic centimetres displacement be allowed in the Gordon Bennett. This is the size of the 50 h.p. Gnome. If the proposition is accepted we will show them whether the biplane is slower than the monoplane. If they insist on motors of any size the race will not interest us, as we do not care to fly over 100 miles an hour ourselves or put our men on such a job."

The following advice may be useful to young members of this Society :—

"It is always a serious thing to land with a strong wind on the back, because as soon as the speed is as little or less than that of the wind, the pressure on the tail tends to turn the machine over instead of preventing it.

"Therefore I would strongly advise with the little machine, that you always keep sufficient height when going with the wind so that you can turn and land facing it. Young birds often get rolled head over heels by making the mistake of landing with the wind. Old birds never attempt it. It will be well for us to follow their example as far as possible."

It is usual with a genius, for the dominant subject to overshadow all others greatly to their detriment. Wilbur did not suffer in this respect. On subjects of which I had some knowledge he could always add some information, and even in my special work I learnt some points from him. Anyone seeking an example of a clear Patent Specification should read Wright Brothers' original specification No. 6732/04. I cannot say which of the two brothers wrote it, because as I have already pointed out, all their work was so combined, but I can assure the members of this Society that the wording is that of the inventors, and only minor alterations are by the patent attorney. Remembering that this subject was unknown and not taken seriously by practical men at the time, the language is wonderfully clear and the features of the invention are marvellously explained and yet in simple language. Nowadays complicated formulæ are considered necessary to explain inventions involving but a fraction of the ingenuity displayed in this master Patent.

Discussing the relative costs of Patent actions in America and in England, Wilbur gave me a humorous example of a Patent Attorney's quotation of the probable costs that would be incurred. On the patentee asking how much it would cost to bring an action for infringement against an alleged infringer, the attorney replied by putting the question, "Well, how much have you got ?" On the inventor giving an estimate of his realisable assets, the lawyer replied : "Well, that's just the amount the action will cost you." Unfortunately there is a great deal of truth in this fable, and he is a lucky inventor who invents a mechanism that is useful enough to be profitable, but not so exceptionally valuable as to tempt all the world to infringe and to belittle the invention in order to excuse themselves. It would be a poor policy if inventors were refused a just reward for

their ingenuity in order to facilitate the free use of their work by all, for the withdrawing of capital and the diverting of intellect from the field of invention into other more secure channels would then result.

When an inventor makes a great invention, he usually sells the foreign rights to the highest bidder. The Wright Brothers had several excellent offers for the British rights, but they did not accept any of these, because they regarded their invention as one carrying moral obligations, and not simply as a negotiable property. They felt it their duty to see that if a company were formed in England, the invention should not be exploited on popular company lines at the risk of the shareholders being left with shares and experience only, in exchange for their money, and as they were unable to withdraw sufficient of their time from their experiments and undertakings in America and on the Continent, they kept the British Patents in their own hands, and have hitherto reaped little profit so far as England is concerned.

This spirit of moderation characterised all their actions. In France rival owners and representatives of other machine makers were welcomed on the flying ground, the machine explained, and in some cases flights given to them. But all their liberality could not save them from the resentment many cherished for the crime of being the first to fly, and so preventing each of those others from being the first if the Wrights had not existed. Fifty years ago men were still on the brink of flight, and with just as much reason, and there is no more justification to expect that these latter men would ever have stepped over the dividing line which their fellow workers of long ago failed to do.

The resentment of this jealousy was afterwards supplemented by the fear of the Wright Brothers endeavouring to collect the tribute which the Crown recognises as the reward of those who give to the nation a new industry, and instead of facing this question in a fair manner many have striven to accuse and excuse, although no application for royalty nor threat of an action for infringement has ever been made against any manufacturer or flyer in Great Britain.

After their return home the Wright Company was formed to work the invention in America, and the companies that had been formed in France and Germany were left to work out their own salvation and to manufacture their own machines. Unfortunately the companies on the Continent were ambitious and missed the guiding hand of the inventors, and many so-called improvements were also introduced into the Continental machines, with the result that while the American company has prospered, the European companies have not progressed. The enthusiasm of this visit to Europe was followed by the natural reaction on the departure of the brothers for America, and the subsequent law actions to enforce the French and German Patents were resented by all who desired to use the invention, but not to pay for it. A campaign of belittlement of the Wrights' invention followed, one claim of anticipation being raised after another, only to be destroyed on investigation being made into its merits. This unfair resistance to a fair acknowledgment of their Patent rights threw a considerable and worrying work on Wilbur Wright, who was thus drawn against his inclinations from his natural work of experimenting and flying, and his health suffered at times from this uncongenial work. A year before his death I persuaded him to throw up this arduous work at Paris and come to England for a change, and some weeks at Eastchurch leading the simple life and working at Mr. Ogilvie's machine enabled us to send him back to the Continent completely recovered. He was in Boston on similar distasteful law work when he contracted his fatal illness, and if he had not been handicapped by lack of out-door life he would probably have pulled through. As it was, he fought for weeks against the relentless microbe that knew no mercy, and he died as he had lived, a fighter to the end undeterred by heavy odds against him.

Although an American, Wilbur Wright was of English descent, and the son of a family of pioneers. One of the Wrights was one of the early settlers at Springfield, Mass., having emigrated from Kelvedon Hall, Essex, in 1636. A later ancestor moved further West, and was one of the first to cut into the forest and found the city of Dayton, Ohio, and Bishop Wright, the father of Wilbur and Orville, has done pioneer work for the Church, whilst a further brother, evidently imbued with the same pioneer instinct, moved out West to Arkansas before his two brothers became famous.

SPECIAL GENERAL MEETING

A SPECIAL GENERAL MEETING of the Aëronautical Society of Great Britain was held at the Royal United Service Institution, Whitehall, on Thursday, May 16th, 1912, for the purpose of amending Rule 27 in respect to Associate Members in such a form as to make a subscription to that grade One Guinea per annum and the entrance fee *nil*.

The CHAIRMAN of the Council, Major-General R. M. Ruck, spoke as follows :—

The object of this meeting is so to amend Rules 27 that *Associate Members will pay One Guinea subscription and be admitted without entrance fee*. The purpose that this object is intended to attain is an increased revenue to the Society. It is perhaps desirable, however, that I deal more fully with this point so as to avoid any possible misunderstanding.

As you are aware, the present membership subscription is Two Guineas with an entrance fee of One Guinea, which amount, although it cannot possibly be considered excessive for those actively interested in the Society's work and able regularly to attend its meetings, does, without doubt, prevent the Society from receiving a more extended measure of support, especially from those in more distant parts of the country who are anxious to encourage the Society's work by joining its ranks at a more moderate fee.

The Council has been aware for some time that this question of subscription has been somewhat of a hindrance to the extension of the membership, which is absolutely essential to the further progress of the Society. Having very thoroughly discussed the matter, the Council came to the conclusion that the best possible course is to make the amendment in the rules that it is the object of this meeting to confirm.

It is true that under the rules as they at present exist, the Council has power to elect Associate Members, who have most of the privileges of full membership, but no power to vote or hold office. At present, however, the subscription to this grade is only half a guinea, a sum that does not repay the Society for the expenses incurred.

It is not anticipated that this proposal will adversely affect the revenue from full membership. This is a Society of scientific investigation, and personal benefits are merely incidental to the process of furthering the common object that all have at heart. Obviously, therefore, all those who can afford to do so should join as full members and all who want to play any part in the control of the Society must so join, because the Associate Membership grade carries with it no vote.

On the other hand, the Associate Membership section should make a strong appeal to many who are anxious to place themselves in closer touch with the movement and desirous of becoming associated with such an honourable and ancient institution as the Aëronautical Society.

All Members of the Society can render much assistance to the Society in this direction, and I must once more emphasise the vital necessity for an increased revenue if the Society is to make the progress that it has set out to accomplish.

With regard to the work in the past, the meetings this session have, I think you will all agree, been very successful, alike in the attendance, in the excellence of the papers, and in the interest they have evoked outside the immediate bounds of the Society. This latter aspect of the situation is one of the most encouraging of all. The fact that we have been able to secure as our Chairmen on these occasions such eminent gentlemen as Sir George Darwin, Sir John Wolff-Barry, Professor Perry, Dr. Shaw, Gen. Sir James Grierson and Mr. F. Grant Ogilvie, who will preside to-night, besides receiving the personal support of Prince Louis of Battenberg, Col. Seely, Sir John French and other distinguished officers, shows how far-reaching is the interest in the work that the Aëronautical Society is undertaking.

But this work costs money, and our funds are quite inadequate. Far be it from us to despise any windfall that may come in the form of a generous donation ; such help has been invaluable to the Society's career in the past. Nevertheless, the Council sees

only one sure policy of progress, which is to make the Society self-supporting from the revenue of its membership. This revenue must be increased ; new members and associate members *must* be obtained.

As regards the future, in order that our efforts may be facilitated and the probability of their success enhanced, the Council is about to publish a small book giving a brief account of the Aëronautical Society and outlining the history of the development of aeronautics since the eighteenth century. The cost of preparing this work has been considerable, notwithstanding the fact that it was written gratuitously and is being printed without profit ; so the Council hopes that every member will not only buy a copy for himself but will buy three or four extra copies (the price will be 1s. each) to send to friends who might be induced to join. I am sure that many people would hesitate no longer if they were fully acquainted with the part that the Aëronautical Society has played in the history of the movement. This little book will tell them something about it, and it will, besides, give the reader an uncommonly clear outline of the principal events that have led up to modern achievements in the air.

When we have more money to deal with, there are a number of things that wait to be done. Already we have in preparation for next session an excellent programme, and the Council is considering the extension of this side of the Society's work by arranging for a series of popular lectures explaining the why and wherefore of various every-day things in aeronautics to those of the public who have only just begun to take an interest in the subject at large. Such a course would, the Council feel, prove extremely popular, and personally I hope that it may be carried to a successful issue.

But, the first thing we have to do is to place ourselves in a position that would enable us adequately to remunerate our Secretary, and you will all be extremely sorry to hear that Mr. Hubbard is leaving us in August. Mr. Hubbard's work for the Society has been a labour of love, for the salary that the Society has been able to give has hardly warranted removing the term " honorary " from his title. We are desirous of finding a new secretary as quickly as possible, and if any member knows of anyone willing to devote the time for this work the Council would be glad to be informed. As I have said, the remuneration at present is nominal, but if the Society's revenue increases, so, too, will the Secretary's income, and a minute has already been passed to that effect.

Another branch of the Society's work that must needs come last although by no means least in importance is that undertaken by the Research Committee. The Research Committee of the Aëronautical Society is perhaps as representative a body of its kind as could well be got together anywhere. But here the Council desires to say that its policy is not to spend the Society's money on any laboratory equipment or experimental ground of its own. There exist already, at the National Physical Laboratory and elsewhere, apparatus for every conceivable kind of research, and it is the Council's object to have its experiments conducted wherever they can be most effectively and most economically carried out. The Research Committee, from the very nature of its personnel, is in an unique position to know what work should desirably be conducted under the Society's aegis ; and there is no fear of overlapping, nor is there any fear of work being undertaken by the Society that might be undertaken otherwise without putting the Society to any expense. Personal service can do a great deal to help the Research Committee. Funds are essential, of course, but personal service is likewise invaluable so that anyone in a position to assist in this kind of work will be helping the movement by putting himself in touch with the Committee in question.

I have explained these things at some length, because now is the time when there is such a great interest in aeronautics that the Aëronautical Society should securely establish itself for all time to come. With these remarks, therefore, I will at once pass the business of the meeting by putting the motion, of which you have received due notice.

A MEMBER : How will the present Associate Members be affected ?

The CHAIRMAN : They will not be affected. They will remain as they are.

The CHAIRMAN then put the motion to the meeting, and it was unanimously adopted.

GENERAL MEETING

THE SIXTH MEETING of the Forty-seventh Session of the Aëronautical Society of Great Britain was held at the Royal United Service Institution, Whitehall, on Monday, April 29th, 1912, at 8.30 p.m. Dr. W. N. Shaw, F.R.S., occupied the chair, and before introducing the lecturer, made a sympathetic reference to the late Prof. A. L. Rotch, whose death on April 7th was so deeply deplored in the meteorological and aeronautical world.

AERIAL TOPOGRAPHY

BY CAPT. C. H. LEY, F.R.MET.S.

ABBREVIATIONS

M = Metres
KM = Kilometres
M.S. = Metres per second
H = Height
H.D. = Horizontal distance
H.V. = Horizontal velocity
V.V. = Vertical velocity
H.V.V. = Hydrogen vertical velocity
A.V.V. = Air vertical velocity
D = Direction
G.V. = Gradient velocity
B = Base subtended (metres)
F.L. = Free lift (grammes)
D.W. = Dead weight carried (grammes)
P = Pressure (inches)
t^D = Dry Bulb Temperature (centrigrade)
t^W = Wet Bulb Temperature
t^H = Temperature of hydrogen at start
H.C. = Rise required for condensation
R = Rotation

NOTE.—All velocities are in metres per second.

My only claim to your attention is that I have been privileged to carry out a large number of observations on Pilot Balloons during the past few years, and have endeavoured to apply some general meteorological principles to the results, particularly in those lower strata of the atmosphere that concern aeronauts.

I have taken the height above sea level of 1·5 KM (5,000 feet) as the practical limit of these strata, as it seems to me there is dynamical division in the neighbourhood of this level. Above this division we generally have the free atmosphere, whose motion is controlled mainly by the pressure distribution, whilst we may regard the layers below it as being the chief field of action of secondary forces set up by the varying topography of the earth's surface.

There are several difficulties in this kind of investigation. It is difficult to grasp complete motion and change of motion in space where we can resolve into components in three directions. It is more difficult still to assign causes for these motions when, in addition to the meteorological conditions, we have both horizontal and vertical divisions of the surface to take into account. Then we have also to remember that meteorological laws are very elastic things, and that owing to the number of variables entering, any particular case may vary widely from the mean.

I shall try to separate for your consideration some of the more important evidences and some results which can be generalised into principles.

I will first briefly refer to one or two fundamental conceptions.

(1) **Gradient Velocity.** For any given set of meteorological conditions we can calculate the horizontal velocity of the free wind at any latitude, assuming steady motion. The question is at what height may we expect such free motion to commence? If we say 1·5 KM we shall only be expressing half the truth, because nearly the full gradient wind may commence at a much lower level. It depends upon the locality. Diags. I. and II.* show the velocities found by balloons at different levels in 1911 at a flat coastal station and a hilly inland station, and indicate how frequently at the coastal station nearly the full gradient velocity occurs at a very low level, but how difficult it is to forecast the level at the inland stations.

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* The diagrams show velocities in vertical columns under heights in top horizontal columns.

(2) **Velocity Gradient.** It has been shown at Blue Hill Observatory that the mean velocity-gradient, or increase of velocity with height, is, in the free atmosphere, only about $2\frac{1}{2}$ metres per second per kilometre, a quantity hardly worth considering by the aeronaut. But in the lower atmosphere, like so many other generalizations, this is subject to very considerable variation. The diagrams show that at Blackpool there was usually a thin surface skin of less than 0·1 KM (300 feet) thickness in which there was a very steep gradient, and often sudden transition to the full gradient velocity, whilst at Sellack in Herefordshire there was often very gentle or even no gradient up to 1·5 KM.

Gusts sometimes exceeded the gradient velocity near the surface, whilst sometimes again the velocities fell off up to considerable heights above. So that a mere calculation of the velocity at a given height from the known surface velocity and the theoretical gradient velocity was apt to be wide of the mark.

(3) **Direction.** Then as regards direction. The free atmosphere is supposed to move in the general direction of the isobars, and I find that in the great majority of the balloons this is so. But at Blackpool the lower atmosphere also deviated very little from the isobaric direction when the winds were of any strength, and this deviation was usually righted rather suddenly in such cases at about 0·5 or 0·6 KM (say 1,800 feet). At Sellack, however, there are large deviations in strong winds up to 1·5 KM (see Diag. IX.), with frequent sudden changes between 0·8 and 1 KM.

I have indicated with a cross in Diag. II. those cases in which the lower winds backed as we ascended. There were eight such cases out of 12, so that again we cannot rely on any general rule in the matter which takes no account of topographical influence.

(4) **Oscillation.** Wind as we experience it at the surface always oscillates in velocity and direction, and the velocity oscillations may be some 40 per cent. or more of the mean velocity. But careful balloon observations show that such large oscillation is by no means an intrinsic property of wind, but rather that it occurs at certain places. These are places where a resisting surface is introduced. The earth's surface is an example on a large scale; an aeroplane, the pipe of a pressure-tube anemometer, or the ball attached to a kite's meteorograph are examples on smaller scales. But there are other surfaces, less material but no less potent, at which there are pressure differences and oscillations, and the most important and interesting of these are the levels of division between two currents or portions of a current.

I am chiefly concerned in this paper with these critical levels, and with the way in which the lower atmosphere sets itself and arranges its gradients to suit them, according to conditions, often perhaps imposed from above.

(5) **Vertical Velocity.** An important factor in the consideration is the motion in a vertical plane. We have much theory but very little measurement in this matter. The theories fall under two heads, viz. :—Temperature effects, and Pressure effects. I will not dwell much on the former, partly because of insufficiency of data, and partly because I do not think they have the same importance as the latter.

Now vertical movement is generally accepted as the cause of rainfall, whether it is topographical rainfall or general rainfall. I will call your attention to the theory explanatory of general rainfall. It is a theory of compression of stream-lines either longitudinal or transverse.

If we had two general currents, one a warm southerly one and one a cold easterly one, they would meet one another along a line of division or trough, and the lighter one would be forced upwards by the obstruction of the denser one. Its stream-lines might be considered to be compressed longitudinally. So that we had a V-shaped depression and rainfall over the trough.

Again, if we had from any cause a centre of low pressure and a general current blowing in towards it, its stream-lines would be compressed by convergence and there would be vertical motion and probably rainfall. This may be called transverse compression.

Temperature was of course a factor in the result, but in whatever way we obstructed a wind the tendency must finally be to divert some of the energy and produce motion in a vertical plane.

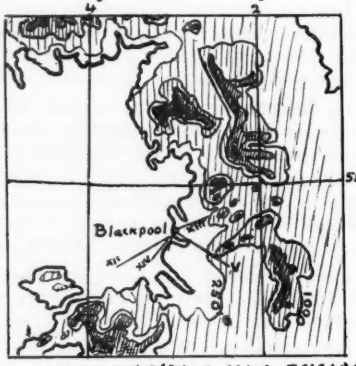
This being briefly the theory of general rainfall, its principles must be applied to topographical rainfall and to the numerous cases appearing from these balloon observations in the lower atmosphere.

I

Date		0 M.	100 M.	300 M.	500 M.	700 M.	D.	G.V.
Mar 9th	P.M.	T	18	17	18	15	WNW	23
" 15	"	5	11	12	12		N	14
Ap. 3	"	T	10	11	11	11	NE	
" 4	A.M.	4		11	12		NNE	
" "	P.M.	6	T	T	11	12	"	14
" 6	"	T	12	11		12	ENE	
" 20	"	6		16	15	16	WSW	14
Ma. 19	"	5	18	11	12	12	NNE	10
" 24	"	6	10	10	8	12	SSW	10
" 29	"	6		10	12	12	E.N.E	12

VELOCITY-GRADIENTS 1911.
(BLACKPOOL)

III

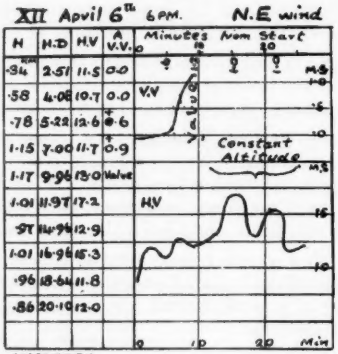


II

Date	·1 KM.	·3	·5	·7	·9	1·1	1·3	1·5	1·7 KM.	D.	G.V.	R.
Ju. 30	4	5	6	6	4					W		—
"	4	5	5	4	4	6	8	10	10	WNW	7	×
July 1.	6	7	7	7	8	7	10	6	7	W by N	7	×
"	4	7	8	8	8	8				"		×
" 3	6	6	4	4	3	2				NW	11	×
"	6	7	5	5	5	5	6	5	6	"		×
" 4	5	6	5	6	4	4	5	6	7	"		×
"	5	7	6	4	4	3	5	6	6	"		×
" 18	9	16	8	8	8	8	8	13	14	W by N	15	×
"	6	7	6	5	9	8	6	9	11	W by S		—
" 31	5	12	9	7	7	8	9			SW	12	—
"		5	6	7	9	10	10	16	13	"		—

VELOCITY-GRADIENTS 1911
(SELLACK)

IV



Orographic Map, N.W. of England. Now let us take a map of the N.W. of England. Diag. III. shows roughly the 250 feet (·08 KM), 1,000 feet (0·3 KM) and 1,600 feet (0·5 KM) contours. There is a large backbone of the Pennine Range covered roughly by a 2,000 feet (0·6 KM) contour, but broken by a narrow broken valley in the neighbourhood of Skipton. There are two large areas on the flanks formed by the Welsh hills and the Lake District each requiring a 1 KM coutour to generally cover, whilst there is a broad opening in Cheshire in the S.E. corner of the area.

If a deep sea covered this area and it was given rapid motion, say, in a west to east direction, the 2,000 feet (0·6 KM) reef of the Pennines would cause a relatively stagnant bottom layer, a controlled layer between 0·6 KM and 1 KM, and a freer current above. There would be an obstructed outlet in the lower layer in the neighbourhood of Skipton and in a N.W. wind there would be a broader outlet over the Dee valley.

Turning water into air and adding heat by solar and terrestrial radiation would greatly modify matters by introducing minor pressure and temperature gradients, greater turbulence and eddy motions. But the main divisions would exist and we should expect some adjustment to them. Compression or expansion of stream-lines according to the direction and velocity of the wind would produce positive or negative vertical motion, and there would be pressure oscillations and gusts at the dividing surfaces of these layers.

Examples. I will take a few examples of different winds at Blackpool in 1911, beginning with some dense N.N.E. winds which had blown from Arctic regions over the 0·6 KM barrier and were moving towards Snowdon.

No. XII. on April 6th (Diags. III. and IV.) shows definite upward motion above 0·6 KM after horizontal oscillation at that level. At 1·1 KM the valve acted and the balloon remained nearly at constant altitude but here showed rather large regular oscillations of horizontal velocity. There must have been some surface deflection along the Welsh coast, as the balloon was picked up in Anglesey.

Nos. XIV. and XV. were sent up in quick succession in completely overcast conditions on May 19th (Diags. V., VI.). Both show general upward motion of air and definite changes at 0·6 KM. No. XIV. moved up in a very even wind and then began to oscillate violently at this level, which it refused to penetrate.

No. XV., after a surface gust, moves up in a slowly increasing wind which oscillated somewhat at 0·6 KM with sudden downward tendency and change of direction. On reaching 1 KM it suddenly gets a wind of 17 metres per second, remains at the same level, and disappears in cloud.

Now these cases are cases of large sheets of stratus cloud in N.N.E. winds. There was no break of cloud at the coast line, the meteorological conditions were not such as to favour vertical motion upwards, and the relative humidity was such as to cause condensation, given an average temperature-gradient, in air which was lifted 0·2 KM (600 feet).

Yet the vertical motion was reliably measured by two successive balloons on May 19th, each showing some condensation and oscillation at 0·6 KM, whilst the general cloud level was not reached until 1 KM. There must have been therefore either super-saturation without cloud, or a great departure of the temperature-gradient from the mean with variation of the humidity condition. Super-saturation is negatived in the layer between 0·6 KM and 1 KM because the later balloon showed no effects of condensation above 0·6 KM.

I came, therefore, to the conclusion, borne out by other cases, that the vertical motions and horizontal oscillations were compression effects produced by the obstructing Welsh mountains in a steady wind already divided at 0·6 KM by the Pennine Range, that the temperature and humidity conditions were most variable at different altitudes and were in a measure governed also by the divisions at which pressure-oscillation and cloud occurred.

On May 24th a strong straight W.S.W. wind was blowing towards the eastern gap in the Pennines (No. XIII., Diag. III.). We found here sudden upward motion with a stronger current at 0·25 KM (800 feet). The vertical velocity drops off but the strong current continues up to 0·6 KM. Above this it decreases in an oscillatory manner with steady upward motion.

This appears to be a case of obstruction up to about 0·4 KM and squeezing of the layer between this and 0·6 KM through the gap between Skipton and Settle with increased velocity.

A good example of an exceptional wind in which there is little obstruction occurred on March 9th (No. V., Diag. III.). A very strong free upper N.W. wind was blowing

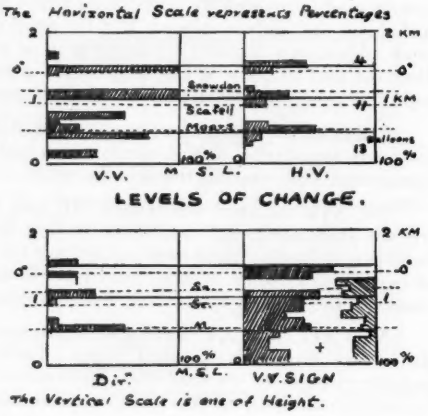
from the North Channel towards Cheshire. There is general downward velocity below it between 1·5 KM and 0·5 KM with decrease of velocity, but a rather stronger horizontal velocity below 0·5 KM, and a thin surface wind from the west.

V N.E. Wind

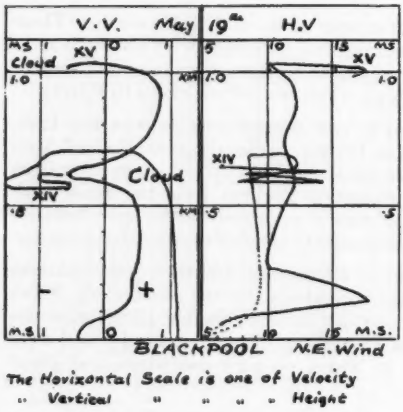
T	H	H.D.	H.V.	H V.V.	A V.V	t^0	t^W	t^n	P
14·28	687	2·56	9·8	+1·75	+1·1	12°	9·2	14°	30·25
16·52	676	3·62	8·8	+1·53	+·88	F.L	=	8°	
18·12	736	4·30	8·6	+1·00	+·95	D.W	=	61°	
19·80	742	5·33	10·9	−·14	−·80	Air	H.C	= ·2	KM
21·55	683	6·80	13·0	−·67	−1·12	Ball	n	= ·6	K.M
23·67	537	7·77	7·9	−1·19	−1·84	Base	=	8·7	m.
25·36	637	9·06	14·2	0·0	−·65	H.V.V.	=	·68	m.s
28·19	565	10·53	8·7	+0·13	−·50	Cloud			
29·62	571	11·87	12·4	+0·09	−·56	Overcast			St.
33·22	571	13·26	6·6	0·0	−·65	Lower	Scud		
34·43	599	13·92	8·15	0·35	−·30	Surface	V.	=	6
34·50	608	15·76	14·5	0·07	·38	G.V = 10			

Picked up at ABERGELE (m.)
XIV May 19th PM.

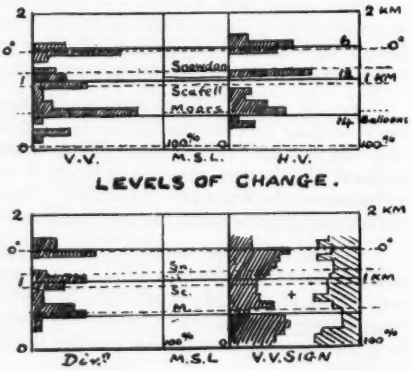
VII BLACKPOOL 1911.



VI



VIII BLACKPOOL 1910



There is no upward motion throughout and the appearance is that of a heavy wind pouring downwards into a hole. (The trough of a V-shaped depression extended from north to south of the North Sea.) Yet the influence of the bordering hills is evidenced in the division mentioned at 0·5 KM, whilst there is no change at 1 KM.

Summary of Blackpool Balloons. I will now show the collective results of all balloons sent up at Blackpool in 1910 and 1911 (Diags. VII. and VIII.). The diagrams show the number of balloons, the percentage of cases showing change of motion, and the height at

which these changes occur. They include all winds irrespective of direction and all definite changes exceeding one metre per second for horizontal velocity and 0·1 metre per second for vertical velocity.

It will be noticed how remarkably the changes, in both years, when they occur, group themselves around the levels 0·5—0·6 KM, 0·9—1·1 KM, and 1·4—1·6 KM.

We have already noticed that the orography of the neighbourhood may account for the two lower levels. But there is no apparent topographic cause for the 1·5 KM (5,000 feet) level. This level is about the mean altitude of the freezing point, and is the level at which the full gradient velocity may be expected. It is also a very common cloud-level. When there were any low clouds to be seen it was fairly safe to say they would be at either the 1 KM or 1·5 KM level, and a glance at the horizon of hills would often determine which. I have on several occasions anticipated the cloud-level as found by the balloon in this way, and also found a fairly accurate value for the velocity at that level by estimating the velocity of the shadow as it passes over the ground. It seems to me that cloud observation is of great importance to the aviator, and that it is a matter of regret that so little advance has been made in this subject.

Sellack Orography. We will now take the inland station. It was situated at Sellack, on a promontory formed by the River Wye, and about four miles N.W. of Ross (see Diag. IX.).

The main features are the Black Mountains and the Breconshire Hills stretching from a point 25 miles N.W. of Sellack to a distance of some 40 miles to the S.W. They shelter the whole of this portion of Herefordshire in westerly winds up to a height of about 0·7 KM (2,300 feet). A nearer western chain of hills, generally about 0·3 KM (1,000 feet) high, and an eastern ridge 0·15 KM (500 feet) high form a basin which is nearly closed to north and south by the Woolhope and Forest of Dean Hills 0·25 KM high. May Hill and the Malverns form outposts further to the east.

The low eastern ridge has however some steep slopes, notably at Ross, Linton, and How Caple, and the diagrams show a few trajectories of balloons passing over these steep portions.

There are several distinct possible causes of positive vertical motion in a westerly wind blowing across this area.

Firstly.—Up to 0·8 KM it is in the wake of the Black Mountains where we might expect some convergence of stream lines from the N.W. and S.W.

Secondly.—Up to 0·25 KM a channel is formed between the western projections of the Woolhope and Forest of Dean Hills, so that the east bank of the Wye between Ross and How Caple would be a line of contraction.

Thirdly.—The steep slope of the eastern ridge near Linton checks the surface wind up to 0·15 KM.

Fourthly.—The downward and upward grazing lines (see dotted lines in Diag. XI.) meet on this east bank.

Fifthly.—The heated air of the valley is piled up in the same locality.

Examples. I will first take a few examples illustrating the effects in N.W., W. and S.W. winds blowing over this area and then give the collective results of the 15 balloons sent up in the hot summer of 1911. (Diag. IX.)*.

Nos. X. and XII. in a light N.W. wind in July, both showed large northward deflection on approaching the steep Ross Hills with preliminary ascending motion. No. X. after this proceeds N.E. to the Linton ridge, where at a height of 1·5 KM it suddenly turns to S.E. into the channel between May Hill and the Malverns (the isobaric direction) with another preceding ascent of air.

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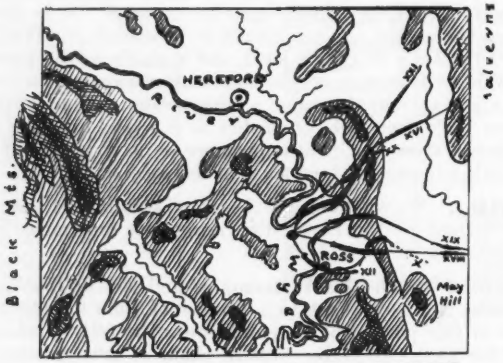
* The trajectory lines shown on the diagram are thickened where upward air motion took place.

Nos. XVIII. and XIX. in a strong west wind on July 18th, showed different directions of trajectory up to 1·5 KM. XVIII. was sent up on a hot evening and showed a very large up-throw of air over the ascending slopes east of the Wye with rapid fall to normal on passing the Linton ridge, and a second rise at 1·5 KM over the channel between May Hill and the Malverns. It was picked up in Belgium at 5 a.m. the next morning.

IX



OROGRAPHY OF HEREFORDSHIRE

X

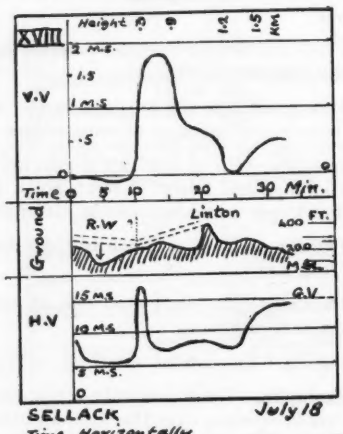
XVIII. 6.15 P.M. XIX 7.30 P.M. JULY 18

H	H.D	H.V.	H V.V	Δ V.V	H	H.D	H.V	H V.V	Δ V.V	
65	2·1	3	·29	0.0	261	1·8	6·1	1·0	0.0	
103	3·0	5·4	·22	−0·1	341	2·7	7·2	·84	−0·1	
144	4·0	6·5	·21	−0·1	415	3·4	6·8	·82	−0·1	
266	5·1	16·7	1·8	+1·5	555	4·3	5·9	1·0	0·0	
880	7·8	7·6	2·0	+1·7	766	5·5	5·3	1·1	+0·1	
1230	10·4	8·3	1·0	−0·7	950	7·4	9·0	1·2	+0·2	
1315	12·9	7·6	·36	+0·1	1190	9·0	8·5	1·4	+0·4	B
1480	15·1	12·5	·72	+0·4	1350	10·2	5·7	·80	−0·2	6.6 m
1670	18·0	13·5	·85	+0·6	1465	11·7	7·6	·70	−0·3	SW
Cloud		(Cu.)			1560	13·5	9·2	·57	−0·4	104
F.L	= 5°		B = 6·6°		1870	17·1	11·7	1·2	+0·2	F.L.
D.W	=	128	9·		1890	19·2	12·2	·17	−0·8	24°

BELGIUM 5 AM
480 KM. .... 11 hrs.

G.V. = 15

XI



SELLACK July 18

Time Horizontally
Velocity Vertically (except Ground Diagram)

The surface wind then changed, the sky was clouded over with cirrus and the temperature fell. No. XIX. took a more northerly course, turned suddenly over the eastern ridge at 0·9 KM, with preliminary rapid ascent, into a S.E. direction, then righted itself with ascent of air at 1·5 KM over the channel between May Hill and the Malverns into the isobaric direction and on the same track as XVIII.

Both balloons showed the effects of compression of the surface wind before reaching the Linton ridge and in the channel north of May Hill.

Diag. X. shows the trajectory tables for these balloons and diag. XI., XII. the synchronous curves of the vertical and horizontal air-motion and sections of the ground passed over. I would call special attention to the violent horizontal gust which ushers in the up-throw of air over the locality of compression (Diag. XI.) also the drop of wind velocity during the period of this up-throw.

Similar effects were found by balloons XXI., XX., and XVI. sent up successively in a strong south-west wind on the cloudless afternoon of July 21st. The vertical motion was in each case marked over the area of compression, and in the earliest balloon of the three, No. XXI., there was seen the violent gust preceding an abnormal up-throw of air. Both this balloon and No. XVI. showed vertical motion at a later stage, which I attribute to heating and compression of the surface air in the valley of the River Leadon felt at heights of 0·75 KM and 1 KM—1·5 KM respectively.

On the assumption that the main effects were due to the throwing-up of the surface winds by the slopes of the ridges I have tried to calculate the result from the formula V.V. = H.V. tan α sin β where α is the slope of the hill, and β the angle of incidence of the surface wind. Diag. XIII. is the result and certainly seems to show some rough agreement between the calculated and observed effects.

Summary of Sellack Balloons. Diag. XV. shows the collective results of the 15 balloons sent up at Sellack in July, 1911, arranged to illustrate by percentages the vertical localities of change. You will notice again how the changes group themselves around the levels 1·5 KM and 1 KM, but that the grouping below is rather spread out between 0·5 KM and 0·75 KM (2,400 feet), whilst there is a very definite stratum of change at 0·3 KM (1,000 feet). The diagram in the right-hand bottom corner shows the vertical distribution of upward and downward air-motions, the blank central column representing upward motion, and the left-hand shaded column downward motion. The surface layer up to 0·3 KM (the height of the western rim of the Wye basin) shows very frequent downward motion, whilst above it, up to 0·9 KM, there is a large excess of upward motion.

The division at 0·3 KM seems certainly due to the following of the ground slopes by the surface winds, which, being from westerly points, had to move initially over downward slopes till the river Wye was passed.

The less definite division about 0·75 KM may be attributed to the western mountains, but it is difficult to attribute the changes about 1 KM to the distant Snowdon Range unless we imagine that the whole of the lower atmosphere in moving across England sets itself into strata for horizontal movement, the dividing line between which is about 1 KM, whilst the topographical vertical effect is felt up to 1·5 KM altitude.

This vertical motion is independent of the heights of the hills, and depends mainly upon the horizontal wind velocity, the slope of the hills, and the temperature of the surface layers.

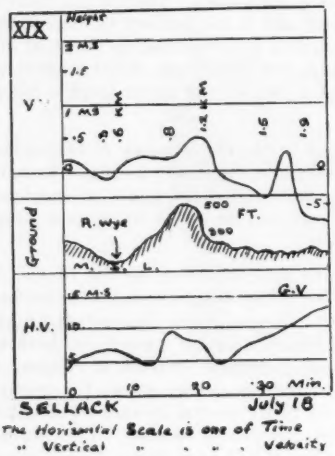
General Levels. On re-examining the results of balloons sent up in 1908 at Limerick and Glossop, I have been surprised to find similar marked tendencies to changes of motion at the three rough general levels 1·5 KM, 1 KM, and 0·6 KM. Although at these places the lowest of these levels can also be attributed to the neighbouring hills, the 1·5 KM and 1 KM level indicate a general tendency to division of strata extending over the whole western portion of the United Kingdom.

Horizontal Localities of Change. As already indicated a great difficulty in the examination of balloon results lies in the two planes in which we may view them. I have dwelt in this paper mainly on the view in a vertical plane by showing that we can group the most important changes at certain general levels and that there are good reasons for attributing this fact to the permanent orographic features of the districts concerned. There is, however, evidence of horizontal divisions and localities on the map such as coast-lines and river-valleys where sudden aerial changes occur; and most winds probably contain places of minor discontinuity and eddy motion, with variation of temperature

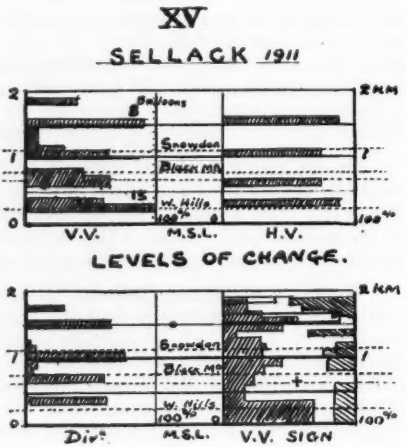
and humidity. I think the more reliable observations at Blackpool and Sellack in 1910 and 1911 show, however, that the most important variations occur over hill slopes and at the orographic levels.

In conclusion I will give you briefly a few details as to magnitudes and permanency of these phenomena.

XII



XV



XIII

CALCULATED HILL EFFECT

Balloon	Wind H.V	Hill sin α	Incr. sin β	+ Air V.V. Calc.	Obs.
III Jul. 30	5 m/s	·21	·42	·44 m/s	·40 m/s
IV	3·5	·10	·50	·17	·10
V Jul. 1	6	·21	·42	·53	·25
VII	4	·21	·42	·36	·40
XVIII Jul. 18	9	·21	·68	1·30	N–1·6
XIX	6	·06	·97	·35	·20
XXI Jul. 21	11	·15	·94	1·55	Large
XVI	4	·15	·95	·57	·55

SELLACK 1911. (H.V. sin α sin β)

Magnitudes. Vertical air-motion from 0 to 0·5 metres per second (100 F.M.) is evidently quite common in the lowest 1·5 KM. Upward or positive vertical motions of the order 2 metres per second and negative motions of 1 M per second are not rare, especially in hot conditions and over hills, whilst sudden change involving a numerical change of 1·5 M. per second is common.

At the critical levels we may get pressure oscillations and cloud with gusts nearly equal to an addition of 100 per cent. to the mean wind velocity, whilst the larger positive vertical motions may be ushered in by violent sudden gusts exceeding this amount. Hot days are favourable for the latter effect and must be considered dangerous to an aviator when the surface breeze is strong.

The positive vertical motions may extend up to the level 1·5 KM (5,000 feet), but the negative motions are comparatively thin, the thinnest ones being of larger magnitude. A thickness of 0·2 KM (600 feet) may be expected for downward motions up to 0·5 M per second and 0·1 KM (300 feet) for those of the order 1 M per second (200 F.M.).

The latter appear also to be of short duration, whereas the former extend over wide areas and may last for some hours.

Both positive and negative vertical motion is commoner in the earlier hours of the afternoon.

Change of direction is sometimes sudden at the critical levels or over ridges, but in winds of the order 10 M per second and over it seldom exceeds 15° of azimuth.

It often occurs synchronously with other changes.

Over inland stations there is more frequent upward air motion between 0·25 KM and 0·6 KM and between 1·1 KM and 1·4 KM than elsewhere, but the negative motions are most frequent on the leeward edges of hill ridges and around the critical levels 0·6 KM, 1·0 KM, and 1·5 KM.

To my mind the above are a few facts which have loomed largely behind many of the interesting discussions we have had at recent meetings of this society.

Discussion

Colonel Rawson said that the three meteorological problems the airman was most interested in at present were—gusts, waves, and vertical turbulent motion. The lecturer had brought before them some very interesting points bearing on the first and third, and Colonel Rawson thought it most probable that he had put his finger on the correct cause for the phenomena he had discovered, in regions which were favourable to vertical turbulent motion. It was not thoroughly appreciated what the effects were of "trajectories" of air impinging on masses of similar air confined against the face of a cliff or hill. An instructive lesson on this point was to be got by lying down on the edge of a cliff, 150 to 200 feet high, with a strong gale of wind blowing into its face. At the very edge a match could be lighted without much trouble, but by leaning forward as far as could be done with safety the gale carried a handkerchief vertically upwards when it was held out in the wind. Again, if you drive along a shore, with a gale of wind carrying the spray 300 yards inland, you will reach a bend in the road where it passes on the seaward side of a rock not higher than 40 to 50 feet and only 70 feet wide. Instead of being wet to the skin by spray at such a spot, as you might expect, there is neither spray nor wind of any consequence to be felt. A buffer of compressed air lies there and the wind must be forced upwards as well as to the sides.

The föhn winds are good examples of the upward movement of air upon meeting the resistance of a mountain range and it is extraordinary what a distance such air will travel under certain circumstances before it descends as an over-poweringly oppressive hot wind. The "Brickfielder" of Sydney and the "Kalahari" wind (really the Berg wind from the Drackensberg Mountains) of Durban are good examples of this.

The author of this paper showed a slide of the upper winds in some of which the rotation was not clock-handed. Colonel Rawson noticed that in every case where the wind backed, the wind that was registered had a northerly component. This was quite in accordance with what he had observed in all the records relating to upper currents. The law seemed to be for England: "Stand with your face to the surface wind and the

rotation in the upper levels will be to your right, except in the case of the north wind, which will come more from your left, both in winter and summer." In fact, the north wind in the upper levels backs, while all the others veer.

He considered that the Society was much indebted to Captain Ley for giving them these valuable original observations which he had made. The more they were studied the greater would their importance be found to be. The training he had received from his distinguished father, Clement Ley, and subsequently in the Corps of Royal Engineers, made any facts regarding aerology which he might contribute, well worthy of their careful consideration.

Captain Ley, in reply, said he did not think that in the lowest 1·5 km any rigid rule as regards rotation of the wind with increase of altitude could be laid down. He could show many instances of north winds veering and of west winds backing in these strata. Above 1·5 km the rule might generally hold, but there were direction -oscillations above which did not appear to conform to any such simple law.

Reference had been made to observations by self-recording theodolites. With every admiration for these ingenious labour-saving devices he thought that the errors introduced by the machinery and the methods were serious when it came to the fineness required for determination of vertical air-motion.

GENERAL MEETING

THE SEVENTH MEETING of the Forty-Seventh Session of the Aëronautical Society of Great Britain was held at the Royal United Service Institution, Whitehall, S.W., on May 8, 1912, at 8.30 p.m. In the absence of General Sir John French, owing to illness, the chair was taken by Major-General SIR JAS. GRIERSON, C.B.

The Chairman, in opening the meeting, congratulated the Under-Secretary of State for War, Col. J. E. Seely, on all he had done for British aviation since his announcement at the Society's meeting in December.

THE DESIGN OF A MILITARY SCOUTING AEROPLANE

BY BRIG.-GENERAL D. HENDERSON, C.B., D.S.O.

THE title of this lecture does not mean, as you may have thought, that I am going to enter into competition with skilled aeroplane designers or harbour intentions of teaching them their business. The purpose is quite different. I desire to put before you what are the qualities that will be demanded in an aeroplane which is used for the purpose of acquiring information in war. I do not wish to praise, or to disparage any particular type of aeroplane as a flying machine. I speak solely of the suitability or otherwise of types as instruments of war.

Before plunging into the subject of the design of an aeroplane designed for scouting it is necessary to consider a little the work which the scouting aeroplane is intended to accomplish, and for this it is advisable that one or two points with regard to reconnaissance in general should be made clear. Reconnaissance, which is quite apart from secret service, has always in the past followed two main methods. The first is that in which a force is sent out to gain information, and is of such strength that it is prepared to deal with any opposition which it is likely to meet, and to fight for its information. The second method is that in which a very small body, possibly only a single man, is sent out in the hope of his evading the enemy's protective parties, and, by stratagem and concealment, making observations of such of the enemy's dispositions as may come within his view.

If you like to go back to the fourteenth century you will find that, on the day of Crecy, King Philip of France, adopting one method, sent out four knights to reconnoitre the English Army and bring him tidings of their dispositions. This reconnaissance was eminently successful. The leader of these four was a man well known (although not perhaps by name) to history; he was Henry le Moyne, one of the knights who linked their reins with those of the blind king of Bohemia, and left their bodies to mark the farthest point that any foeman had penetrated into the English lines.

Ten year later, before Poictiers, we find the Black Prince adopting the other method and sending a force of some hundreds of men-at-arms to observe the French Army. So strong was this party, and so ready to fight for their information, that they charged into the rear of the French main force, and made off with a number of prisoners.

Marlborough used for reconnaissance bodies of cavalry of considerable strength; Wellington trusted mainly to the efforts of single officers, mounted on the best horses that could be procured. Throughout all military history one finds these two methods employed, sometimes simultaneously, sometimes alternately.

Of the first method of obtaining information—by force—it is only necessary to speak here for the reason that the troops which are organised for reconnaissance on a large scale present the most serious difficulties and dangers to hostile reconnaissance on a small scale. Whatever equipment or means of transport may be used by a scout, his most dangerous opponents are those of his enemy who are similarly equipped but in stronger force. The most dangerous enemy of a scout on horseback is a stronger force of the enemy on horseback; if the scout be on a bicycle, bicyclists are his most dangerous

opponents. If a scout be using an aeroplane, his greatest danger will arise from the enemy's aeroplanes, in stronger force. The advantage that a single man or the small body has is that a large force is usually slower, more visible, more noisy, and more cumbersome.

A scout has always two duties to perform and two difficulties to overcome. He has first to get information, and secondly he has to bring it back. To be successful he must carry out both. But while he may do valuable work although he carries out the first duty only partially, he must carry out the second completely. Incomplete information is of some value if delivered to the person who can make use of it, but the best information is of no use if the scout who obtained it is killed or made prisoner before he can pass it on.

Therefore it is that, so long as the means of transport used by a scout does not preclude the possibility of acquiring information, the most important requirements of the animal, or vehicle, or vessel, or machine, for scouting purposes are first, the possibility of escaping observation, and second, suitability for eluding the enemy, if discovered. For example, a horse used by a scout should be, for the first purpose, well trained, not nervous, not given to neighing; for the second purpose it should be fast, a stayer, a good fencer, and handy. If the scout should have to carry out his operations from a boat or vessel of any kind, such vessel, whatever power may be used to propel it, should be silent, it should emit no visible smoke, and it should be of a dull colour; for the second purpose it should be fast, it should be able to cover long distances, and it should be handy.

If you will consider any possible means of transport which might be used by a scout, I think you will come to the conclusion that the four essentials, always allowing a certain facility of observation, are invisibility, silence, speed, and manœuvring power.

On these lines one may consider the requirements of an aeroplane for scouting work which may have to evade a number of machines designed for fighting. It seems to me that the problem will present many similarities to the problems of reconnaissance that have had to be considered by all commanders of troops from the earliest days.

There is another valuable attribute, which I only mention to show that it is not forgotten. The range of an aeroplane depends on its speed, the trustworthiness of its engine, and on the amount of fuel which it can carry. With regard to the engine, you must get the engine of the best design and the best workmanship that is procurable; coddle it like a baby; feed it on the best petrol and oil, and put your trust in Providence. As to the amount of fuel and lubricant to be carried—that depends on the particular duty to be performed. I would not dare to guess at a standard duration of flight for which fuel might have to be carried, but I am inclined to think that for some time it will not be necessary to exceed, in war, the records already attained in peace.

I have spoken, so far, about methods of reconnaissance which have been approved in the past, for the reason that I am anxious to get a firm starting-point, a fixed point of departure, before entering on speculations concerning the future. For any consideration of aeroplane reconnaissance is at present speculation; we have no data as to the behaviour of either man or machines when tried in war in face of opposition of the same type. Of the possibilities of aerial reconnaissance when there is no aerial opposition we can speak with some certainty; not only can the limitations of hostile action from the ground against a well-flown aeroplane be calculated with some accuracy, but we have also a certain amount of practical experience, gained in Tripoli, to assist us. That problem presents but little difficulty and is capable of definite solution. But the problem of warfare in the air is altogether a different matter. It is not susceptible of definite calculation, and we have no practical experience to guide us. We can only speculate; and to keep our speculations within reasonable bounds, we can use analogies from other methods of reconnaissance; we can use knowledge of war and of human nature in war; we can apply mechanical knowledge; and, most important of all, we can try to use common-sense.

The beginning of my speculation, as you may have guessed, is that it is probable that two types of aeroplanes will be evolved for military purposes: a fighting machine and a scouting machine. This is only an opinion; it is possible that these two types may merge into one; it is possible that they may be diversified into half-a-dozen. But, judging by the analogy of reconnaissance on the ground, these two types will be found necessary. The fighting aeroplane will be required to overcome the air forces of the enemy; if possible to drive them to the ground; at any rate, to inflict damage upon them, and, in spite of opposition, to penetrate far enough to gain accurate information of the dispositions of the enemy's troops. Also it will be required to block the enemy's endeavours to gain information by bringing to action any of his aeroplanes which may approach our lines. To fulfil these purposes the fighting aeroplane must be of such design that weapons can be used effectively by the passengers or pilot.

Here I may say, quite frankly, that I have not seen, nor have I heard of, any aeroplane that can be considered a fighting aeroplane. There are certain types that could, without much alteration, be adapted to the purpose, but it does not seem that any designer has yet endeavoured to produce a machine of this kind. Designers, in fact, have been busy enough producing machines that will fly, and fly well and safely, without thinking of the exact use to which they might be put in war. And the fighting machine is not going to be easy to design. When you consider the use of weapons from an aeroplane—and by weapons I mean real man-killing guns or rifles, not childish devices for dropping bombs, or grenades or crackers—you will see what limitations it imposes. No tractor screws, I am afraid, and your fighting passengers placed in front of all planes and struts and wires and stays. Then lifting power for three or four men with weapons and ammunition, and lastly, speed sufficient to give some chance of manœuvring on equal terms with your adversary.

I should like to say here, that when I talk of the enemy, I take it for granted that the enemy is as well equipped, in personnel and material, as we are; that his pilots are as skilful and as brave, and his aeroplanes as fast and as powerful, type for type.

I have been talking for a long time, and you may be wondering when I am coming to the subject of the lecture, the Scouting Aeroplane. There were a good many preliminaries to be cleared up first. But now having got so far as to give the opinion, which is very widely shared, that a real fighting aeroplane, for action against other aeroplanes, is certain to appear, and that before long, we can go on to discuss the probability of another type being used, and to guess at its probable characteristics. And in the first place I think we may safely say that the tendency for the moment, is towards concentration in one type, owing to a certain limit that has been reached in the matter of speed. Racing aeroplanes are already attaining speeds at which landing becomes rather a hazardous operation, and we cannot look for any great increase in air speed until some method is devised of limiting speed on landing, or some new principle is evolved in the design of landing chassis.

At the same time the speed of heavy machines, approximating to the strength and power which will be required for fighting machines, is constantly increasing. If, therefore, we can get a heavy fighting machine to reach the limit of speed for safe landing, there is not much room for the light unarmed machine until some means have been devised of increasing air speed while limiting speed on landing. I will admit that there is likely always to be a certain margin of possible speed in favour of a very light aeroplane in regard to this matter of landing. A bad landing may be less disastrous with a light machine than with a heavy one, and it is also possible, in an unfettered design, to minimise the danger to the pilot in the event of a smash on landing. It is therefore possible that when the limit of speed for the safe landing has been attained by fighting aeroplanes, this limit may be exceeded by scouting machines which are built to smash safely, if I may put it so. This method of attaining increased speed would, however, I imagine, be not very generally used, and it is by no means certain that the margin of speed thus obtained would justify the use of a purely scouting aeroplane against fighting machines. For the required speed of an aeroplane for purely scouting purposes is not a fixed quantity,

it is relative; the speed must be higher than that of the enemy's armed craft; the greater the difference in speed between them the better. Therefore I would suggest that the first item in the list of requirements of a scouting aeroplane must be variable speed, for without that, or some entirey novel landing arrangement, I can see no place in war for anything except the fighting craft.

If, however, the landing difficulty can be surmounted, then there is a very important rôle for the scouting machine. For there seems no reason to doubt that a machine built specially for speed can be made materially faster than one built specially for fighting; in the case of the scouting machine there are no real limitations of design. You may have any kind of screw or propeller you like; you may hang half-a-dozen propellers round the machine if you like; you can have engines as powerful as you like, if only they are not too noisy; you may carry a passenger, or only a pilot; you may carry weapons or go unarmed; in design generally you have a free hand. It seems to me that, under such conditions, it is axiomatic that such a machine can be made faster than the fighting aeroplane, the design of which is limited in so many ways.

The main use of the scouting aeroplane will probably be for preliminary reconnaissance; to mark down large bodies of troops, to observe changes in their disposition; perhaps, also, to observe the dispositions and movements of the enemy's aeroplanes. The light fast machine would be used for these purposes because of its speed, which would give the pilot a good chance of escaping such hostile machines as were sent against it. If a solitary *fighting* machine were sent on such an errand, it would probably be met by a superior force of aeroplanes of its own type, and it would be too weak to defeat them and would have no advantage in speed by which to escape them. When fighting aeroplanes are used they should be sent out in such strength as will give them a fair chance of defeating or holding off the hostile machines that are likely to be brought against them; they are not for solitary use.

In dealing with an aeroplane as a means of transport for a scout, one striking advantage of air reconnaissances becomes apparent. On the ground, a scout who is discovered by a superior force has only two dimensions on which to escape; he is confined to the surface of the earth. The flyer can escape in three dimensions, and if his climbing power is superior to that of his adversaries, he will be very hard to catch.

I have taken speed firstl, because it seems to be the most important of the attributes of a scouting aeroplane. Again I would say that speed is only relative, and that a sufficient speed means a sufficient margin of speed over your adversaries. It does not seem probable that the problem of speed will reach finality for some considerable time.

The three other requisites I have mentioned are silence, invisibility, and manœuvring power. Manœuvring power is in so far as the machine is concerned a matter of stability and efficient control; it is also a relative quantity and the standard will depend on the success which designers may achieve in overcoming the clumsiness of the heavier fighting machine.

Invisibility and silence, on the other hand, are positive virtues; and the nearer we can approach to complete invisibility and perfect silence, the better. Invisibility may be approached by judicious arrangement of surface material and colour; and the prevention of noise is not a very complex engineering problem.

The design for a scouting aeroplane, therefore, should make provision for the four essentials: speed, which includes climbing speed, manœuvring power, silence and invisibility, and should give certain facilities for observation.

In selecting these attributes as essentials, I have not been guided by any knowledge either of aeroplane construction or of the art of flying. My experience in both directions is paltry, and I disclaim entirely any right to speak from either point of view. The subject has been attacked solely from the point of view of reconnaissance in war. Bearing this in mind, we might now consider some attributes which I would term non-essentials

A certain possibility of observation is an essential; the scout must be able to see something; but an absolutely unrestricted view is a non-essential. It is desirable, certainly, but only if it does not interfere with another essential. There are machines now existing which give a practically perfect view, but lack the other essentials of a scouting machine, and I think any insistence on this attribute would tend to cramp design. To insist on an absolutely unrestricted view, to the detriment of speed or manœuvring power, would be like sending a scout out on foot carrying a step ladder in order that he might set it up, when he found the enemy, in order to get a good view.

Another non-essential, more or less desirable in a scouting machine, is power of arising from rough or sticky ground. This is convenient, of course, but hardly of first importance. For a machine starting from its own lines the most careful preparations can be made, and, with regard to re-starting a machine which has been forced to descend in the enemy's lines, I think it safer for the present to assume that such a mishap implies the failure of the enterprise as an aerial reconnaissance, and that bad starting ground must be looked on in the same light as a broken chassis or a defective petrol pipe. The scout in such case must be prepared to abandon his aeroplane and make the best of his way home by other means. Many a man has started on reconnaisssance on his good horse and has come back on his bad feet, and aeroplane scouts need not expect to escape similar trials. Of course, if we can get a machine to rise from a snipe-bog, so much the better, but it cannot be called an essential.

Suitability for landing well, on the other hand, is so much of an essential that I have looked upon it as a preliminary to the very existence of a separate type of scouting machine. A good gliding angle is a convenience, and an additional safety; and I am inclined to venture the opinion that, mathematically, it is closely connected both with speed and manœuvring power, and will improve proportionately to these two attributes. If, however, I should be wrong in this view, a flat gliding angle is still a non-essential and must not be sought after to the detriment of more important requirements.

Protection or armour of any kind for the aeroplane is at present, I should guess, a non-essential. The scout must trust to his superior mobility, and there can be no doubt, I think, that any serious attempt to provide protection would impair mobility.

The question of whether a scouting aeroplane should carry two men or only one is to my mind a non-essential. If two men can be carried without detracting from speed and climbing power, then undoubtedly it is an advantage to have an observer with nothing to do but observe. On the other hand, it is better to have a reconnaissance succeed with one man than fail with two. I will leave it at that.

The proceedings of a scout in war are of course infinitely varied, according to the circumstances under which he finds himself. Let us consider, however, for a moment the operations of a scout who has been told to verify the positions of certain of the enemy's troops, and let us suppose that a circle of fifty miles by aeroplane will be necessary.

He might very well start before dawn, get his altitude, and circle about his own lines until it was clear enough to observe accurately. Then, with noiseless engine and aeroplane coloured so as to show as little as possible against the sky, he will make for the enemy's position. It is quite possible that he may complete his half-hour trip without being discovered. Or he may be discovered from the ground, in which case he ought to have finished his reconnaissance before any fighting machine could climb sufficiently to put him in danger. Or he may be discovered by an enemy in superior force already in the air, in which case he will possibly, though not certainly, have to retire with his work incomplete. Or he may be intercepted, in which case a circuitous chase or a climbing contest may ensue. It seems to me, however, that such a scout will have a good chance of escaping the enemy's observation altogether, and I can see a great prospect of success for reconnaissance conducted in this fashion.

There is one point, that I am sure has occurred to many of you, on which I have not touched; that is, the case of two scouting aeroplanes, of similar type, opposing

one another in the air. I can only say that, judging by the action of single scouts on the ground, it is unlikely that conflicts will occur very frequently between such craft. Their business is scouting, not fighting; they are not equipped for fighting. If they fight they are likely to attract undesirable attention to themselves, and if they are determined to fight, they must get to very close quarters. If the machines are at all equally matched in speed and handiness, the scouts might manœuvre round each other for an indefinite period, missing each other with pistols, or endeavouring, and failing, each to give the other his wash. The use of weapons, on a craft not designed for it, will be very difficult, and the probability of a scout determining on self-destruction, in order at the same time to destroy his enemy, is remote.

I have tried to suggest to you the lines on which the requirements of a scouting aeroplane should be considered. Very likely you may not agree with me on all, or on any of the points I have put forward. But this lecture will have served its purpose if only those designers who have heard it will be able to satisfy themselves not only that they disagree, but why they disagree.

In many conversations I have had with people connected with flying, when discussing different types of aeroplanes, I have heard different machines described as " the very thing for scouting," or " splendid for reconnaissance," and I have said " Yes, splendid," without much enthusiasm. In nearly every case, the obvious virtue in the machine which was pointed out to me, was what I call a non-essential, and speaking of reconnaissance generally, inexperienced people are apt to attach undue importance to non-essentials. I have tried to give you the benefit of a small experience of scouting on the ground, but I am quite alive to the possibility that such experience may be but a poor guide to the art of scouting in the air. In the air, it is possible that experience will prove all our theories fallacious. As the old scout said, " The only way to learn reconnaissance is to go out scouting, and the only way to learn to scout is to go out on reconnaissance." But until we get our experience of reconnaissance in the air, we must try to utilise our experience of other kinds of reconnaissance, and work out our salvation by thinking, and by discussing, and by trying experiments.

Discussion

Col. J. E. Capper, C.B., R.E., said that a view all round the machine was not inconsistent with speed and ease of manœuvre. Also, he did not agree that a light, fast machine was unable to attack a heavy machine, as it might oust the heavier machine by quick manœuvring. He thought air scouts might fight. On land, scouts, if they met at all, met between armies, not behind ; but, if a scouting plane were observed over an army, he thought one would always find officers willing to take up another scouting machine to prevent the enemy from getting back with information.

General Arbuthnot said that he was of the opinion that the scout should be independent of the pilot, chauffeur, or whatever one would call him, so as to be able to concentrate on his own work. He thought that more attention should be paid to engines, especially to silencing. Makers of aeroplanes were too liable to fit engines they knew, instead of experimenting with the best engines.

Captain H. F. Wood, referring to the question of colour, said that if a machine went up in the grey dawn and arrived over the enemy in bright sunlight, it might be difficult to change its colour to suit the altered conditions. Attempts made in France to give aeroplanes protective colour had been unsatisfactory, as they always looked white when high up. Silencing of the engines was being attained with success.

Captain C. J. Burke said that there are two points to which he would like to draw attention. The first was that the best of machines would be useless without someone to fly them. After the struggle in the air the condition of the nerves of the surviving fliers

on the defeated side would probably prevent them flying at all. Success in the combat in the air would therefore seem all important, and every effort should be made to gain it before giving attention to anything else.

The second point was that in the existing aeroplane there was the defect that a certain amount of muscular effort was necessary to control it in bad weather. Aeroplanes on service would have to fly long distances in bad weather, consequently the muscular effort required to control the machine must be made as small as possible.

Mr. A. R. Low remarked that the designer of an aeroplane was in every way more restricted by properties of the materials with which he worked than the naval designer. When the naval designer received greater claims from the naval officer, he could go to the Exchequer for a larger grant, and by increasing the displacement of his hull he could obtain greater protection, speed and gun-power. With the aeroplane the contrary was the case. If he might repeat the gist of remarks made at a previous meeting, and supposed that a light single-seater was equipped with the best motor (and for such motors at present, the 50 H.P. Gnome set the standard); if they further supposed that the aerodynamical and constructional design took complete advantage of all available materials and methods, then an interesting question arose: What were the properties of a family of geometrically similar aeroplanes? that is to say, suppose all the linear dimensions were varied, then what would be the variations of the stresses supported, and the resistance of the material employed?

Take a simple numerical example. Supposing that all the dimensions were doubled, then the weight and the consequent stresses would increase eight-fold, while the cross-sections of the various members of the frame would increase only four-fold. In the result given identical quality of material and construction, the large machine would have only half the coefficient of safety possessed by the smaller machine. That was a law of nature from which the designer had no escape.

Those interested in Aviation had pushed on the Government and the Naval and Military Authorities to believe that the aeroplane was a magnificent weapon. And so it was. It was only an apparent contradiction if he raised a word of warning, lest too much was expected of the new instrument of war. He would conclude his remarks with the obvious inference that the limitations referred to were entirely in favour of the defence.

Captain F. H. Sykes said that in Tripoli the best distance done by a scouting aeroplane was 90 miles out and 90 back, but it must be remembered that the Italians had nothing up against them. In future much bigger flights would be needed. In Tripoli, bomb-dropping was tried at first, and found useless, but after giving it up for two months it had recently been started again. The Italians first flew at a height of 2,500 ft., but had lately come down to about 1,500 ft.; only one aviator had been hit, and then it was because he had been brought down to 900 ft. by a down current.

Colonel H. E. Rawson, C.B., R.E., said that petrol might be good, but it was not the ultimate power producer. He had seen a car recently which had run some thousands of miles on liquid air, and he learned that the chief trouble about it was that the intense cold caused the metal of the engines to fracture under shocks. This could probably be cured by using new metals.

Colonel Holden said that with a dirigible one had better chances of observation. It was difficult, in any case, to be sure what one thought one saw was the real thing. Observers could easily be deceived by simply turning the surface of the ground over to give it the shape, when seen from above, of guns or other things. At present one had very little knowledge of what one could see at 40 to 60 miles an hour. As to silence in engines, the best known engine, the Gnome, was the most difficult to silence. Still, no engine at full power could be absolutely silent.

Mr. J. W. Dunne agreed with Captain Burke that the first thing in war would be a big aerial battle, but a machine six miles per hour faster than a fighting machine would

be quite safe, and could go clean through the fighting fleet. If the pilot had a fairly clear view, a second observer would not be essential, and silence was not essential if it was going to hamper speed.

Mr. M. O'Gorman said that he had no doubt of the value of speed to a scout. If one put an effective 80 horse-power into a scout, one could get 100 m.p.h., but with the same engine in a fighting machine one would only get 60 m.p.h. The speed of the scout would, however, be limited by his landing speed. He speculated that a 200 m.p.h. machine might be evolved, and in this country it would never be very far from the sea as a landing place.

Mr. Handley Page, on the subject of coloration, said that in the sky the machine always intercepted light and would be visible, but that on the ground one could paint it the colour of the ground. If one was going to land at high speeds, some form of brake on the chassis would be needed.

Mr. Ledeboer said that silencing the engine was not the only source of noise, as the whistling of the propeller could be heard at a great distance, though this had been reduced in France by making propellers with flexible trailing edges. However, the machines were always seen long before they were heard, He questioned whether it was necessary for a pilot to return with information. If the machine were fitted with " wireless," one could communicate observations, and this also did away with the need for landing.

Brigadier-General Henderson (in reply) : I will endeavour to answer the questions that have been put in as much detail as is possible in the time at our disposal.

Colonel Capper considered that a view all round an aeroplane was not inconsistent with speed. This may be so ; but, judging by the machines at present in existence, those with a view all round seem to attain a speed of about 90 kilometres an hour, while those which have not a good view can get to something like 90 miles an hour. I do not think that the better view is worth this difference in speed.

On the question of whether a light fast machine could successfully combat a large armed machine, I am inclined to stick to my original view, that an aeroplane designed for fighting will always beat off one which is not so designed.

General Arbuthnot's opinion, that the scouting aeroplane should always carry an observer, is one with which everybody will agree, provided the carrying of an observer is not going to interfere with the efficiency of the machine in other respects.

On the question of colour, raised by Captain Wood, the experiments hitherto conducted have certainly not shown that aeroplanes, when in the air, can be made invisible by any system of colouring. The experiments however, have not been very complete, and I think that much better results may be obtained by further experiments conducted on scientific lines. Even the question of colouring on the ground has received very little attention, and this is a problem which is not difficult of solution.

Had I been lecturing on fighting aeroplanes, I would have found myself quite in accord with the opinions expressed by Captain Burke. But before the enemy's aeroplanes can even be attacked they must be found, and for this purpose alone a scouting aeroplane would be required in addition to fighting machines.

I have no particular preference for any form of power producer, whether petrol, heavy oil, or some other fuel. Liquid air, which has been mentioned, has many apparent advantages, but there are also many well-known difficulties in harnessing it to an engine.

Colonel Holden has drawn attention to the difficulty of observing when traversing the air at great speeds. It must be remembered, however, that the greater the altitude the less the difficulty of observing at high speed. Observation in war must be carried out at considerable height in order to be reasonably safe. An ordinary military rifle gives

an effective vertical range of about 7,000 feet, but an aeroplane is such a difficult target that a flyer may be considered reasonably safe at heights over 3,000 feet. At such heights a speed of 60 miles an hour would add very little to the difficulty of observation.

It has been suggested that an aeroplane would always be seen before it was heard ; but those who have experience of sleepy sentries in war will know very well that, in the dawn, a silent machine might very well escape observation. You would be surprised at the number of things a sentry does not see.

The question of landing speed is one of great difficulty. But it is important to remember that, in a successful reconnaissance, only one landing is absolutely necessary. That is the landing of the scout on the conclusion of his reconnaissance in order to make his report. If he should smash his machine in this landing it matters very little, so long as he does not smash himself. It has been suggested by Mr. Ledeboer that if aeroplanes are fitted with wireless even this landing would be unnecessary, as the report could be transmitted from the air. It seems to me, however, in the first place, that the scout must land some time, and that he must have some sort of chance of coming to the ground without breaking his neck. In the second place, although wireless is theoretically an attractive method of getting over all the difficulties of the transmission of intelligence, yet its use is surrounded by difficulties of other kinds. The enemy can intercept messages, or interrupt messages. Messages may also be interrupted by other wireless communications between bodies of our own troops. There are many difficult problems to be solved before wireless can be considered an effective and trustworthy means of communication on the battlefield.

Major-General Sir Jas. Grierson then proposed a vote of thanks to the lecturer, which was heartily rendered.

Colonel the Rt. Hon. J. E. B. Seely, D.S.O., in proposing a vote of thanks to the Chairman, said that, speaking as a student of military aeronautics, he would ask designers of aeroplanes to bend their minds in the direction of producing machines of automatic or inherent stability and variability of speed. Prince Louis of Battenberg had pointed out a few months ago the need the Navy had of " web-footed hawks." Hawks had one quality an aeroplane lacked—they were able to hover. Let the designers produce an aeroplane that could hover, be silent and invisible, and transmit and receive wireless messages, and the whole of warfare would be revolutionised.

An immense fortune was waiting for a man who would produce such a machine.

Speaking as the responsible head of the Committee which controlled the aeronautical defences of this country, so far as it was in the power of the Government, they would strive their utmost to make this country foremost in the air as it was on the sea.

THE AERODYNAMIC RESISTANCE OF BARS, STRUTS AND WIRES—III.

Made at East London College (University of London) for the Research Committee of the Aëronautical Society of Great Britain.

BY A. P. THURSTON, B.SC., A.F.AË.S.

(*Erratum.—The resistance of all the bars, struts, turnbuckles, &c., given in the last* communication, with the exception of the circular rod* ·85 in. *dia., are reduced to the air density of* ·00127 *and not* ·00122 *as stated.*)

The aerodynamic resistance of wires. The apparatus used in these experiments was described by the author in his †Report to the Laboratory Committee of the Aëronautical Society of Great Britain. When the experiments herein described were started in 1910 no information on the resistance of wires and ropes was available, but the following papers have since been published :—(1) " The Resistance of Wires and Ropes in a Current of Air," by B. Melville Jones, B.A. ; (2) " Comparison of the Resistances of Stationary and Vibrating Wires," by T. E. Stanton, D.Sc., M.I.C.E. (Report of the Advisory Committee for Aeronautics, 1910–11) ; and (3) " The Tests of Smooth Wires and Wire-ropes made at the Göttingen " Modellversuchsanstalt " (Ztsch. für Flugtechnik und Motorluftschiffahrt, Oct. 29th, 1910). Owing to the absence of funds the experiments of the resistance of wires have not been carried to a conclusion. The following data and conclusions are derived from a series of preliminary or trial experiments. They are published herewith for the guidance of future experimenters.

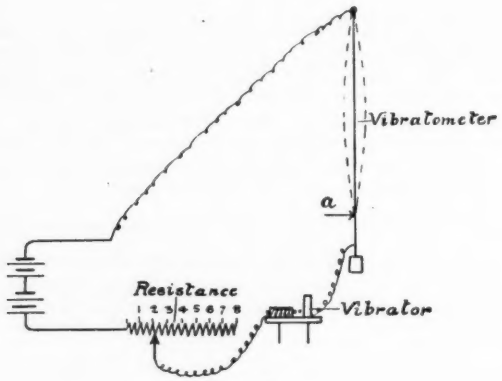


FIG. 3.

Apparatus for vibrating the wires. Figs. 1 and 2 show fuller details of the vibrating device than those previously published. The balance is mounted to one side of the tunnel which is turned at right angles to the position used for determining the resistance of struts. Two arms or levers project into the tunnel from the side and are protected by wind shields. One of these arms is rigidly mounted to the balance and the other is fulcrumed on a vertical knife edge so that the tensions of the two wires are identical. The electromagnet and vibrator are mounted on a table adjustable vertically and horizontally under the action of a fine screw. The vibrator is shown in Figs. 2 and 3 and consists of two platinum wires, *a*, *b*, insulated from each other and connected in the circuit of the electromagnet. The longer wire, *a*, contacts with the specimen wire and its outward

* *The Aëronautical Journal*, April, 1912. No. 62, Vol. XVI.

† *The Aëronautical Journal*, April 1911, No. 58, Vol. XV, p. 65 *et seq.*

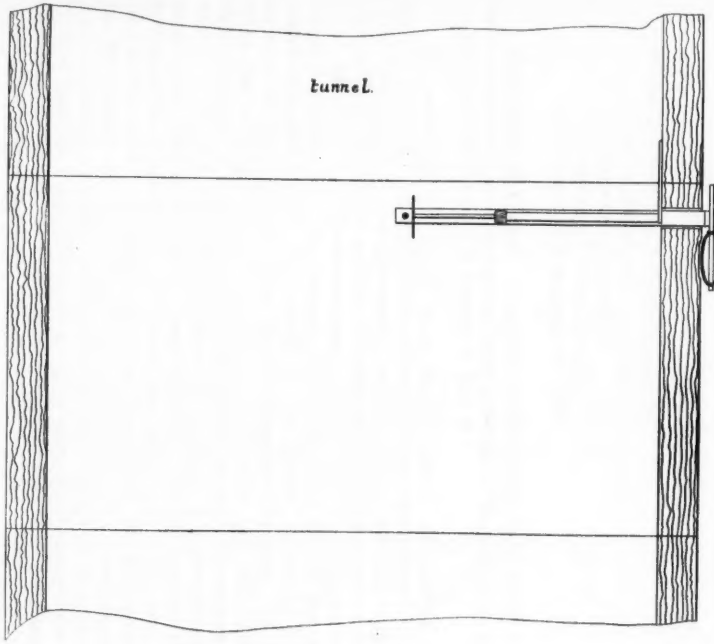


FIG 1

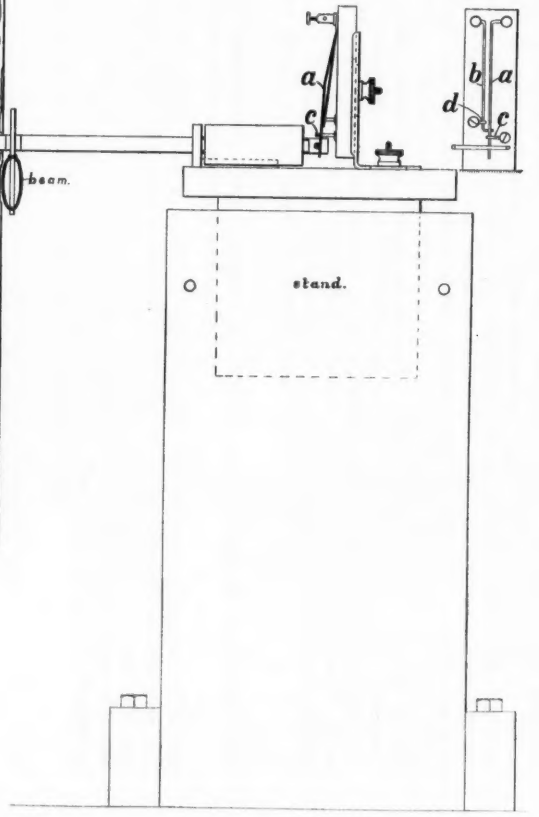


FIG 2

ARRANGEMENT OF VIBRATOR.

motion is limited by a stop, *c*. The end of the shorter wire, *b*, is bent at right angles so as to lie in the path of the wire, *a*, and its outward motion is limited by the stop, *d*. When the wire, *a*, is carried inwards by the vibrating wire it makes contact with the bent portion of the wire, *b*, and the circuit is closed. The wire is therefore vibrated in the well-known way and the position of the vibrator may be adjusted to obtain the best amplitude. The frequency of vibration is varied by altering the tension of the wire by means of adjustable screw clamps at the ends. This vibrator works well but in any future experiments it would be modified so as to make and break contact on both sides of the vibrating wire. The portions of the vibrating wire in and out of the tunnel are connected by a thin wire to ensure both portions vibrating together and to the same amplitude. The amplitude of vibration of the wire in the tunnel is limited by an adjustable stop having a short length of vertical wire soldered to the end.

The Vibratometer. The frequency of vibration was determined in the following way as shown diagrammatically in Fig. 3.

A known weight was hung at one end of a thin wire of known diameter. The top and bottom of this wire were connected in the circuit of the vibrator and the wire was hung in the field of a permanent magnet. An adjustable bridge, *a*, contacted with the wire and was moved up or down until the vibratometer wire vibrated in unison with the test specimen. The length of the vibrating portion was then measured and the frequency calculated from the well-known formula for vibrating wires.

$$n = \frac{1}{2l}\sqrt{\frac{T}{\pi r^2 \rho}}$$

Where n = number of vibrations per sec.
l = length of wire in inches.
T = tension in poundals.
r = radius of cross section.
ρ = density of material of wire.

Exploration of Tunnel. A number of tests were made along the centre of the longitudinal cross section of the tunnel with different arrangements of gauze, bellmouth, and curtain and the results are plotted in Fig. 4.

The same notation is used as in the first report. The tunnel was made continuous an l the position C is therefore non-existent in these experiments. Curves 1 to 4 inclusive were determined by the difference in reading of two dynamic tubes, one being fixed at the centre of the tunnel and the other moved along the centre of the tunnel. The right hand side of all the curves correspond with the side of the tunnel remote from the knife edges of the balance. The increase of velocity at the sides of the tunnel due to the *vena contracta* caused by the wind shields is well shown. It will be noticed that a peak is obtained in the position at which the two dynamic tubes abut. As this peak was thought to be due to the influence of one tube upon the other, one tube having a larger diameter than the other, the curves 5 and 6 were drawn for the same arrangement of gauze as in curve 1. Curve 5 was obtained from the readings of a movable dynamic carrying the static tube with it, and curve 6 was obtained from the readings of a fixed static tube and movable dynamic tube, the static tube being fixed in the centre of the longitudinal wall of the tunnel. In these curves the centre peak has disappeared. The liability of the Pitot tube to error due to small external influences is thus demonstrated. The curves 5 and 6 correspond with a fair degree of accuracy except at the outer portions and appear to show that the Pitot tube is not accurate when the dynamic tube is situated in certain regions of the tunnel. It would therefore appear that an investigation into the limitations of the Pitot tube is desirable.

In the following experiments the arrangements shown in connection with Curve 1 was used and the velocity was determined by means of a fixed dynamic tube at the centre of the tunnel in conjunction with a fixed static tube in the longitudinal wall of the

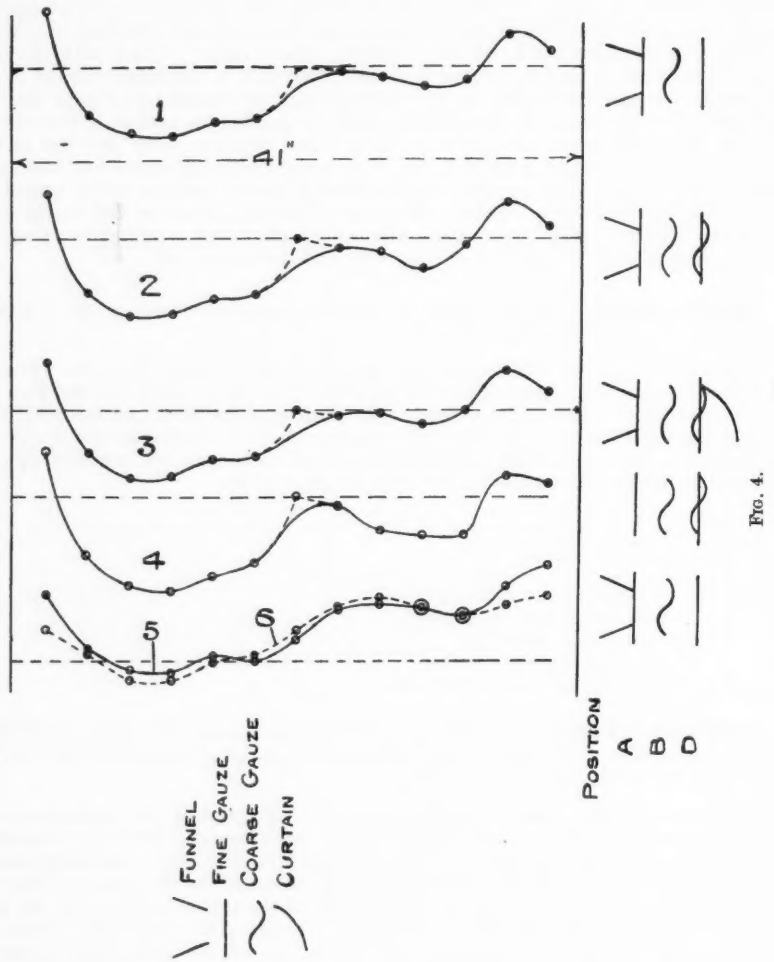


FIG. 4.

tunnel. This curve gives a maximum variation of velocity of 5·48 per cent. It was the intention of the author, after the trial experiments hereinafter described, to place baffle-plates in the tunnel and to dispose the wire gauze in such a way as to obtain a uniform current before making the final experiments. The velocity distribution shown will give a rather excessive value of the resistance of the test wire since the centre of pressure will be slightly to one side of the centre of the wire. This error could be in part eliminated by turning the tunnel through a right angle into the position originally used in the last communication and by modifying the method of mounting the specimen on the balance to correspond.

The relation of the diameter of a circular rod or wire to its resistance. The value of K in the equation $R = KldV^2$ was obtained for bars of the dia. ·85 in., ·61 in., ·42 in. and ·202 in. at velocities between 15 and 25 miles per hour with the apparatus arranged as

in the last communication. The value of K for wires of the dia. ·102 in., ·0475 in., ·0325 in., ·0185 in., and ·014 in. was determined with the apparatus arranged as herein described. The results are plotted in Fig. 5.

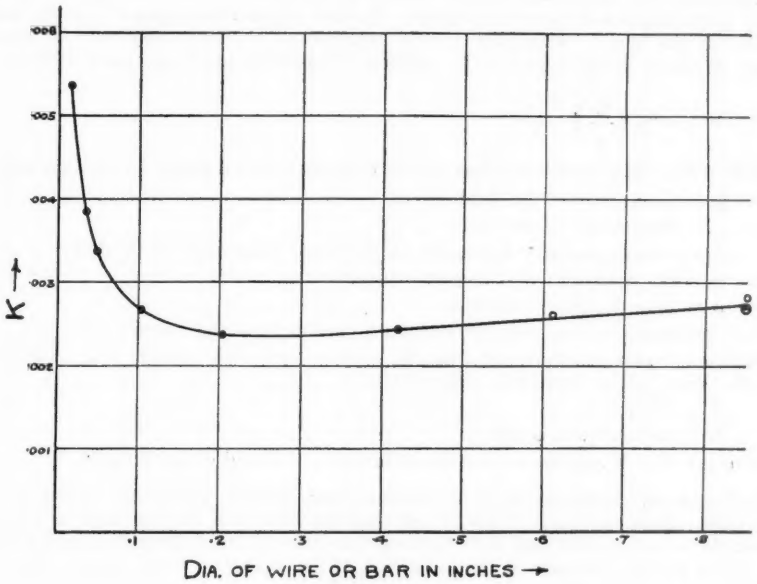


FIG. 5.

It will be seen that between the diameters of ·1 in. and ·85 in. and by inference from Fig. 9, page 79 of the First Report,* up to the diameters of 1·75 ins. the value of K is approximately constant and varies between ·00235 and ·0028. For diameters below ·1 in. dia. the value of K increases rapidly. The inferences to be deduced from this fact are obvious and numerous. It should be stated that throughout these experiments, as also in the previous reports, the air has been taken at the density of ·00127 to correspond with the highest values likely to be met with in flight.

The following table gives some of the values of K obtained :—

TABLE I

Dia. of bars or wires in inches.	Value of K in lbs., feet, and miles per hour.
·014	·00535
·0325	·00384
·0475	·00339
·102	·00268
·202	·00238
·42	·00243
·61	·00261 to ·002675
·85	·002642 to ·00282 according to velocity

The effect of velocity on the value of K and the principle of Dynamical Similarity. It had been noticed on many occasions throughout these experiments that, although the

* *The Aëronautical Journal*, April, 1911, No. 58, Vol. XV.

apparatus was highly sensitive, it was impossible to repeat a value of K unless approximately the same velocity was used. This would appear to indicate either: (1) that the resistance of a bar does not vary strictly as the square of the velocity; or (2) that the reading of the Pitot tube does not vary strictly as the square of the velocity; or (3) that there is a combination of these two effects. The last explanation appears to be the more probable of the three. According to the principle of Dynamic Similarity as stated by Lord Rayleigh in the Report to the Advisory Committee for Aeronautics, 1909–10,

$$R = \rho v^2 \phi \left(\frac{lv}{\gamma}\right)$$

Where R is the pressure at either of two similarly situated points on the two bodies,

ρ is the density of the fluid

v is the velocity of the body

l is a linear quantity depending on the linear dimensions of the body

γ is the coefficient of kinematic viscosity

ϕ is some unknown function.

It is assumed that the velocity v is much smaller than the velocity of sound.

Since ρ and γ are constants and l may be replaced by D this equation may be written, as in Mr. Jones' paper previously referred to,

$$R = Dlv^2\phi\,(Dv)$$

K therefore equals ϕ, (Dv)

that is to say that K appears to be a direct function of D and of the velocity.

In Fig. 6, the values of K have therefore been plotted against the values of the product Dv. From this curve it will be seen that the value of K may be taken as varying between the values of ·0024 and ·0028 between the values of Dv of ·25 ft. secs. and 2·31 ft. secs. From further readings in the possession of the author it would appear that the same holds up to the values of Dv of at least 4·6 ft. secs.

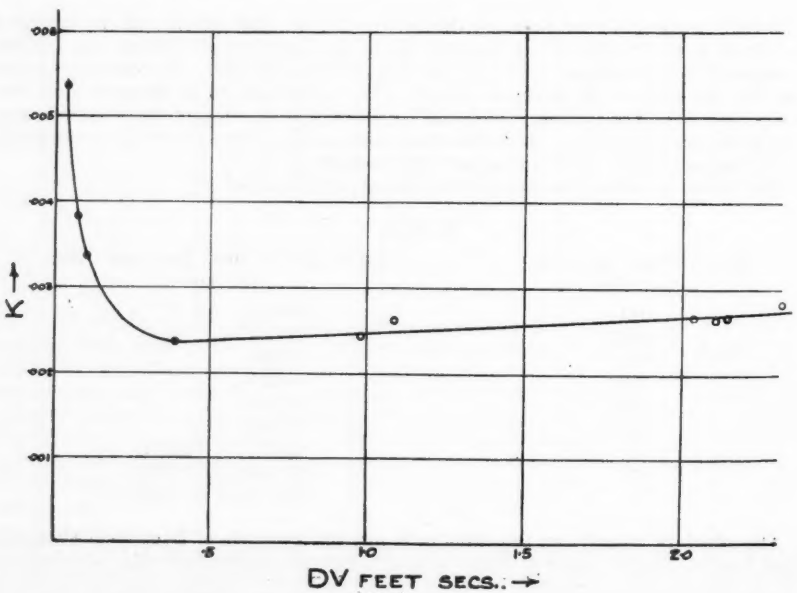


Fig. 6.

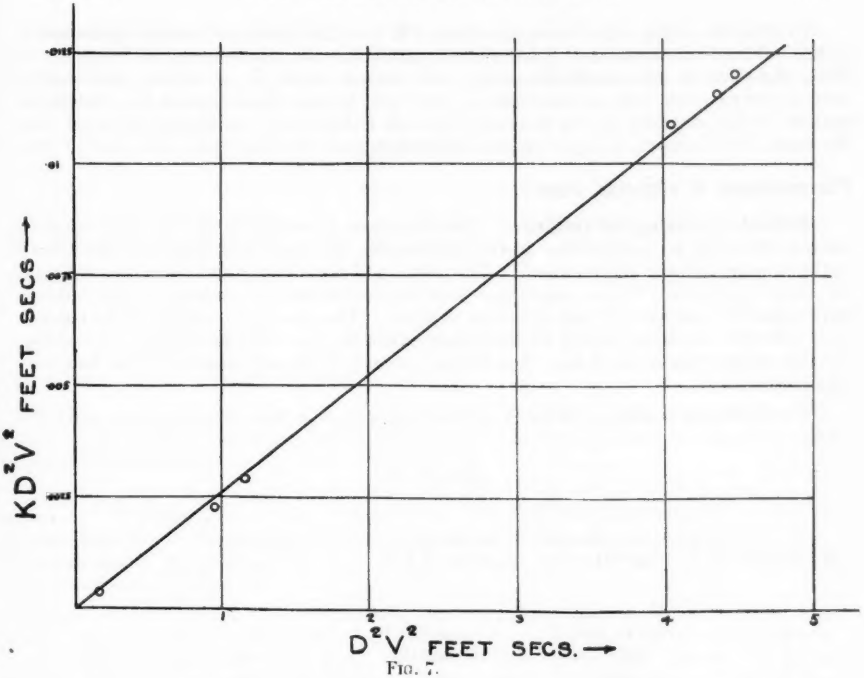

KD²V² FEET SECS →
D²V² FEET SECS. →
1
2
3
4
5

FIG. 7.

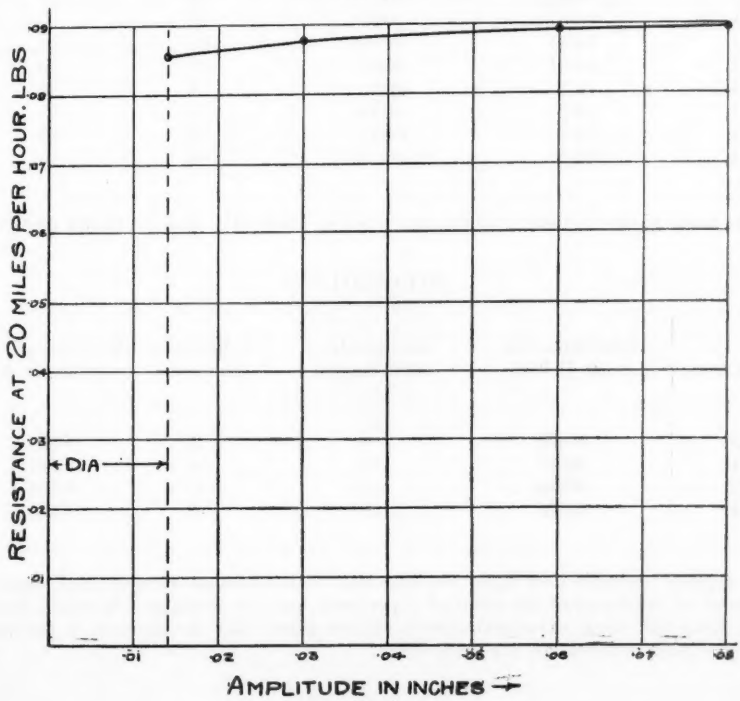

RESISTANCE AT 20 MILES PER HOUR. LBS
DIA
·01
·02
·03
·04
·05
·06
·07
·08
·09
AMPLITUDE IN INCHES →

FIG. 8.

In order to obtain a good average value of K for all the bars and wires experimented on the values of the resistance R have been plotted against the values of D^2V^2 in Fig. 7. From the straight line we obtain as the best average value K = ·00264. Reduced to normal temperature and pressure and to feet per second this becomes k = ·00117 as against ·00107 obtained at the National Physical Laboratory. It should be noted that the value ·00117 covers a larger range of diameters and velocities than the value ·00107.

The resistance of vibrating wires:

Method of making the readings. The amplitude of vibration of the wires could be altered while the air current was flowing by varying the resistance, Fig. 3, in the circuit of the vibrator and the vibratometer. The position of the slider on this resistance was read for each amplitude. These amplitudes and the corresponding readings of the balance were taken in a current of wind of known velocity. The standing readings of the balance with the wire vibrating in still air were then taken for the same positions of the slider. By this means the error, if any, due to the contact of the vibrator with the test wire was eliminated.

The following readings, Table II., were obtained for a wire ·014 in. dia. and 41 ins. long:—

TABLE II

No. of tests.	Speed of current. M.P.H.	Resistance lbs. at 20 M.P.H.	Amplitude in inches.	Vibrations per sec.
4	15·8 to 16·6	·00853	0	0
1	16·2	·00915	·08	15
1	16·25	·009	·06	15
1	16·51	·0087	·03	15
1	16·51	·00887	·06	15
1	16·51	·00887	·08	15
1	16·5	·0087	·06	15
1	16·51	·00896	·03	15
1	16·5	·0092	·06	15
1	16·5	·00885	·08	15

The mean values of these readings are given in Table III., and are shown plotted in Fig. 8.

TABLE III

No. of tests.	Resistance lbs. at 20 M.P.H.	Amplitude in inches.	Vibrations per sec.	K in lbs., ft., and miles per hr.
3	·00896	·08	15	·00562
4	·00894	·06	15	·00561
2	·00883	·03	15	·00554
4	·00853	0	0	·00535

It appears probable from these readings that with vibrating wires of small diameter an increase of resistance of the order of 5 per cent. may be obtained. It would further appear, from additional experiments with thicker wires, that the increase of resistance due to vibration is much less for these wires.

The resistance of vibrating bands. The resistance of a band § in. wide, ·047 in. thick and 41 ins. long when vibrating and not vibrating is given in Table IV. The band was vibrated so as to give a twist to the wind with the object of obtaining the greatest increase of resistance.

TABLE IV

No. of tests.	Resistance lbs. at 20 M.P.H.	Amplitude in inches.	Vibrations per sec.	K in lbs., ft. and miles per hr.
4	·025	0	0	·00468
2	·0287	·2 in. to ·25 in.	9·03	·00537

Future experiments with vibrating bands would be facilitated by mounting the band as previously suggested at right angles to the axis of the balance and by fixing the vibrator to the balance.

As this paper was going to press the attention of the author was directed to the paper on " Windkräfte an ebenen und gewölbten Platten. Drahtwiderstand," by Dr. O. Föppl Aachen (Ztsch. für Flugtechnik und Motorluftschiffahrt. Heft 5, March 16th, 1912). The similarity of the shape of the curve in Fig. II. of Dr. Aachen's paper to Figs. 5 and 6 in this report are striking.

In conclusion, the author would acknowledge his indebtedness to Mr. E. A. Matzinger for designing the balance and to Mr. E. E. Farrant, B.Sc., for much painstaking work in connection with this communication. He would also thank Prof. Morris for much valuable assistance and the loan of electrical apparatus. Thanks are also due to the Governors of East London College for permission to carry out this work in the Aerodynamical Laboratory at the College.

THE AERODYNAMIC RESISTANCE OF BARS, STRUTS AND WIRES—IV.

Made at East London College (University of London) for the Research Committee of the Aëronautical Society of Great Britain.

BY A. P. THURSTON, B.SC., A.F.AË.S.

The shielding effects of struts and bars. In the previous communications, I., II., and III., the aerodynamic resistance of single bars, struts, and wires was investigated. The object of the investigation herein described was to determine the shielding effect of one bar mounted in the direct path of another bar of identical length, shape, and size.

Method of mounting the specimens. The upper bar was suspended from the fork of the balance at two points 6 inches apart in the manner described in communication II., and the identical lower bar was pivoted to the upper bar by two parallel wires 6 inches apart. Means were provided to allow the lower bar to be gradually lowered away from the upper bar. The axis of the bars were kept parallel and the ends of the two bars were kept exactly beneath one another throughout the experiments. All the bars were 24 inches long and $\frac{5}{8}$ inch thick.

The shielding effect of one strut, rod, or bar upon another. The total resistance of two circular rods each $\frac{5}{8}$ inch diameter and 24 inches long is shown plotted in Fig. 1 against the gap or distance between the rods.

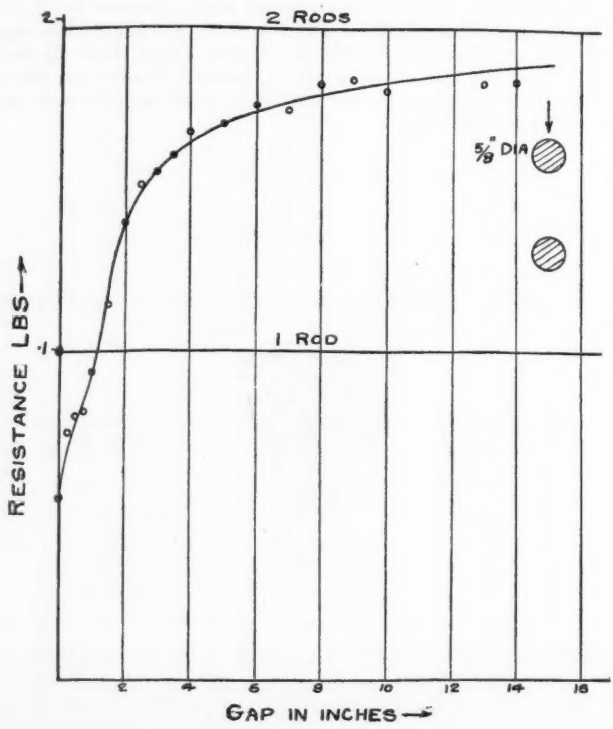


FIG. 1.

When the rods are in contact the total resistance of the combination is only about half the resistance of one of the rods alone. As the gap is increased the resistance increases rapidly, and when the rods are about $1\frac{1}{4}$ inches apart (i.e., 2 diameters apart) the resistance of the two rods is equal to the resistance of the single rod. When the rods are 20 inches apart (i.e., 32 diameters apart) the total resistance of the combination appears to be about 6 per cent. to 7 per cent. less than double the resistance of the single rod. By reference to Fig. 6 of communication III. it will be seen that the value of the resistance of the single rod appears to be between 3 per cent. and 4 per cent. high for the velocities used in these experiments. It may therefore be assumed for practical purposes that the resistance of the combination, when the rods are 20 inches apart, is double the resistance of the single rod.

A corresponding curve for square rods of $\frac{5}{8}$ inch side is shown in Fig. 2.

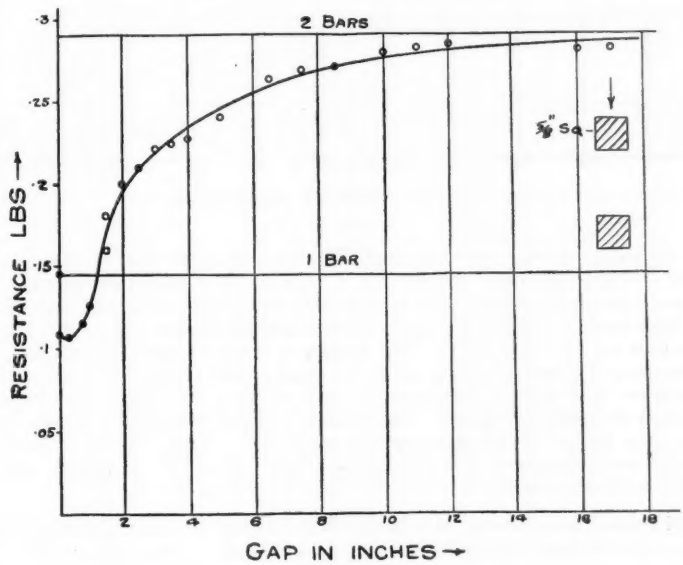


FIG. 2.

The total resistance of the combination, when the two bars are in contact, is only about three-quarters the resistance of one of the rods alone. As the gap is increased there is at first a slight decrease in the resistance. When the gap is $\frac{5}{8}$ inch (i.e., the thickness of the bars) the total resistance is the same as when the bars are in contact. The resistance rapidly increases as the width of the gap increases. When the gap is $1\frac{1}{4}$ inches wide (i.e., twice the thickness of the bars) the resistance of the combination equals the resistance of the single bar. When the bars are 10 inches apart (i.e., 16 times the thickness) the total resistance of the combination is only about 5 per cent. less than double the resistance of the single bar. It would appear that the resistance of the combination may be assumed to be double the resistance of the single bar when the distance between the bars is not less than 20 inches (i.e., the gap is not less than 32 times the thickness). For all readings with a gap greater than 13 inches the bars swayed badly in the tunnel, thus showing an unstable condition in the flow of the air. A close examination of the readings appears to show that the resistance follows a wave path about the mean curve. As these readings repeated this would

appear to indicate a periodic change in the form of the air flow with certain increases in the air gap. This wave-like increase of resistance is shown more clearly in Fig. 3 for diamond-shaped bars $1\frac{7}{8}$ inches deep and $\frac{5}{8}$ inch thick.

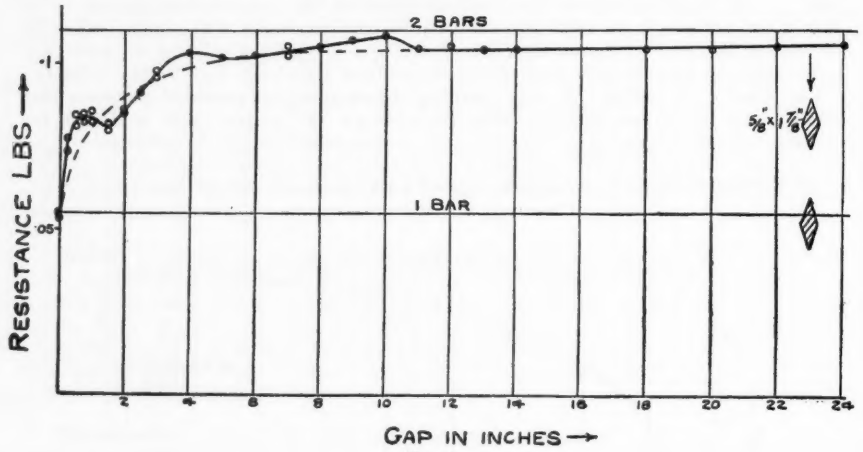


FIG. 3.

The resistance of the two bars when in contact is approximately the same as the resistance of the single bar. As the gap is increased the resistance increases at first rapidly and afterwards slowly until a maximum is reached, when the bars are 1 inch apart. With further increase of the gap the resistance decreases and reaches a minimum when the bars are $1\frac{1}{2}$ inches apart. The resistance increases again and reaches a second maximum when the bars are about $4\frac{1}{4}$ inches apart. The resistance then decreases and again increases until a third maximum is reached, when the bars are 10 inches apart (i.e., 16 times the thickness apart). The resistance of the combination is then only 2 per cent. less than double the resistance of the single bar. With a further increase in the gap the resistance decreases and afterwards slowly increases. A curious agitation in the air flow is thus revealed.

Fig. 4 shows the resistance of two bars in which the section is formed of two arcs of circles. The section is $1\frac{7}{8}$ inches deep by $\frac{5}{8}$ inch thick.

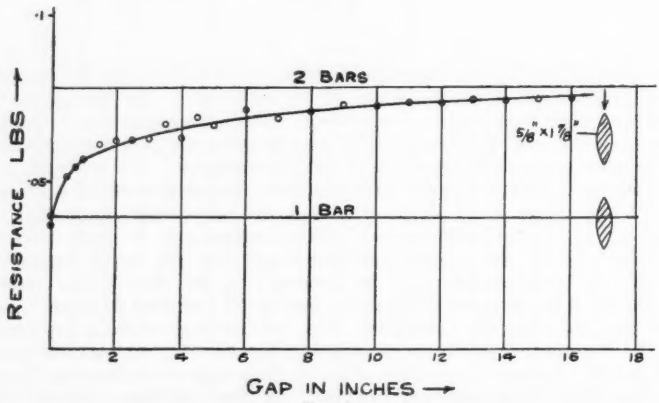


FIG. 4.

The resistance of the combination when in contact is slightly less than the resistance of the single bar. As the gap is increased the resistance increases, and when the bars are 14 inches apart (i.e., nearly 22½ times the thickness apart) the resistance of the combination is only about 5 per cent. less than twice the resistance of the single bar.

The resistance of bars having sections formed by drawing tangents from a point 1¼ inches away from the circumference to a circle ⅝ inch diameter are shown in Fig. 5 with the " blunt end " and the " point " to the wind respectively. These bars are for convenience called circular based triangles. They are ⅝ inch thick by 1⅞ inch deep.

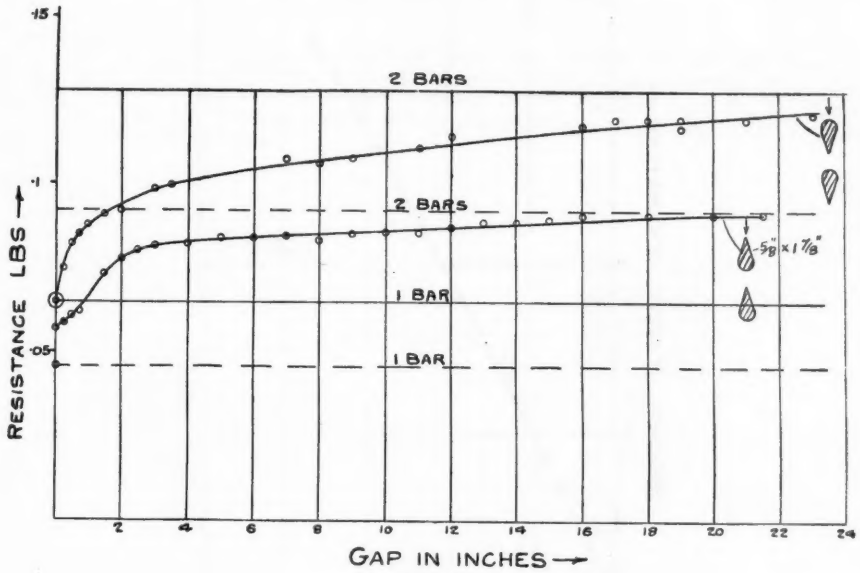


FIG. 5.

The upper curve in Fig. 5 shows the resistance of the two bars when both have the " blunt end " to the wind. The lower curve shows the resistance of the two bars when both have the " thin end " to the wind. The resistance of the two bars " thin end " to the wind is considerably less than the resistance of the same bars " blunt end " to the wind for every width of air gap. Additional evidence in support of the author's statement that the resistance of most shapes of bars is least when the thin end is to the wind is thus provided.

With the bars " blunt end " to the wind the resistance of the two bars in contact is approximately the same as the resistance of the single bar. The resistance of the combination increases with increase of the air gap. When the bars are 20 inches apart the resistance of the combination is about 5 per cent. less than double the resistance of the single bar.

With the bars " thin end " to the wind the resistance of the two bars when in contact is about 1¼ times the resistance of the single bar. The resistance gradually increases with increase of air gap, and when the bars are 20 inches apart the resistance of the combination is only about 1 per cent. less than double the resistance of the single bar.

As the result of all the above experiments it may be assumed to within an error not greater than 5 per cent., and probably considerably less that *the total resistance of struts following in the same " run " of air and more than* 30 *times the thickness apart is the same as the total resistance of the same struts each in a clear run of air.*

The relation between the resistance of a strut and the velocity. It is generally assumed in problems connected with aerodynamic resistance that the resistance varies strictly as the velocity. In the communication III. it was proved that this is not strictly true. For most bodies the index n in the equation $R=AV^n$ is slightly less than 2.

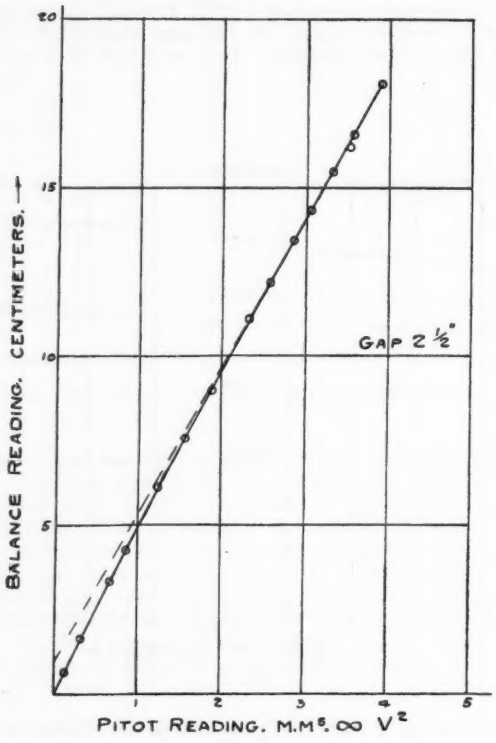


FIG. 6.

Fig. 6 shows the curve obtained by plotting the resistance against the square of the velocity for the bars used in Fig. 4 with an air gap of 2½ inches. If n was strictly equal to 2 the curve in Fig. 6 would be a straight line. It will be seen from the curve obtained that the value of n is slightly less than 2, and that the deviation is greatest at the smaller velocities. The dotted line shows a tangent drawn to the upper portion of the curve. By taking logarithms of the readings and plotting the results in the well-known manner the value of the index n over certain ranges of velocity may be obtained. This will be done when the results of the author's experiments fully treating of the variation of the value of the index n are published.

In conclusion, the author would thank Mr. E. E. Farrant, B.Sc., for valuable assistance in making the observations and the calculations connected therewith.

The author would also express his thanks to the Governors of East London College for permission to carry out this work in the Aerodynamical Laboratory at the College.

TENSILE TESTS ON STEEL TAPE, WIRES, AND FLEXIBLE STEEL CABLE

(Presented to the Research Committee of the Aëronautical Society of Great Britain)

BY W. G. MANN AND J. BRIMELOW

The following tables are compiled from tests carried out at East London College on various materials of aeroplane construction, kindly supplied by Messrs. W. N. Brunton and Son, T. W. K. Clarke and Co., F. Handley Page, and H. Rollet and Co. Figure 1 shows the method of fixing the steel tapes during test. Table I. gives the results of these tests.

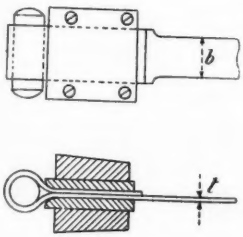


Fig 1.

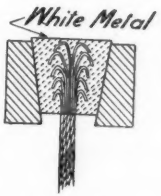


Fig 2.

TABLE I

No.	Description.	Thickness *t*.	Breadth *b*.	Maximum load.	Maximum stress.
		in.	in.	lb.	tons per sq. in.
1	Blériot, $\frac{3}{4}$″ × 18 W.G. ..	0·0495	0·69	4,050	49·0
2	Ditto	0·0495	0·69	3,990	49·4
3	Ditto	0·0495	0·766	4,500	53·0
1	Blériot $\frac{5}{8}$″ (·55″) × 18 W.G. ..	0·047	0·55	3,090	53·1
2	——	0·047	0·55	3,090	53·1
3	——	0·047	0·55	3,230	55·4
1	Blériot, $\frac{7}{8}$″ × 16 W.G. ..	0·0635	0·8	4,930	43·3
2	——	0·0635	0·8	4,930	43·3
3	——	0·0635	0·8	4,930	43·3
1	Clarke $\frac{3}{4}$″ (·7″) × ·0285″ ..	0·0285	0·7	2,170	48·6
2	——	0·0285	0·7	1,900	42·6

Table II. gives the results of tests on steel wires of different diameters. Some of the specimens were plain, others galvanised. The results are marked accordingly. The wires were secured in ordinary wedge-shaped tension grips.

TABLE II

No.	Description.	Diameter.	Area.	Maximum load.	Maximum stress.
		in.	sq. in.	in lb.	tons per sq. in.
1	Plain	0·129	0·0130	2,975	102·0
2	No. 10	0·129	0·0130	2,975	102·0
3	W.G.	0·129	0·0130	2,975	102·0
1	Galvanised	0·1025	0·00825	2,050	110·1
2	No. 12	0·1025	0·00825	2,050	110·1
3	W.G.	0·1025	0·00825	2,050	110·1
1	Plain	0·0915	0·006575	1,725	117·0
2	No. 13	0·0915	0·006575	1,725	117·0
3	W.G.	0·0915	0·006575	1,725	117·0
1	Galvanised	0·0755	0·004476	800	80·0
2	No. 15	0·0735	0·004242	775	81·5
3	W.G.	0·0735	0·004242	825	87·0
1	Galvanised	0·0635	0·003167	690	97·5
2	No. 16	0·0635	0·003167	700	99·0
3	W.G.	0·0635	0·003167	700	99·0
1	Galvanised	0·0475	0·001772	375	95·0
2	No. 18	0·0475	0·001772	375	95·0
3	W.G.	0·0475	0·001772	375	95·0
1	Galvanised	0·035	0·000962	225	104·5
2	No. 20	0·035	0·000962	225	104·5
3	W.G.	0·035	0·000962	225	104·5
1	Plain	0·0315	0·000779	175	100·0
2	No. 21	0·0315	0·000779	175	100·0
3	W.G.	0·0315	0·000779	175	100·0
1	Plain	0·0280	0·000615	120	87
2	No. 22	0·0280	0·000615	120	87
3	W.G.	0·0280	0·000615	120	87

Tables III., IV. and V. give the results of tension experiments on flexible steel cables of varying strengths. Figure 2 shows the method of fixing.

TABLE III

Size (a) 12 wires round a core of 7.
Dia. of each wire = ·009 in.
Total area of metal = 0·00121 sq. in.

Specimen.	Breaking load.	Breaking stress.
	lb.	tons per sq. inch.
1	200	73·9
2	190	70·2
3	170	62·8
4	210	77·6
5	200	73·9
	Mean 194	Mean 71·7

TABLE IV

Size (b) 12 wires round a core of 7.
Dia. of each wire = 0·016 in.
Total area of metal = 0·0038 sq. in.

Specimen.	Breaking load.	Breaking stress.
	lb.	tons per sq. inch.
1	660	77·1
2	720	84·1
3	680	79·5
4	660	77·1
5	690	80·6
6	775	90·6
	Mean 697·5	Mean 81·5

TABLE V

Size (c) 6 strands of 6 wires each round a core of 7.
No. of wires 47. Dia. of each wire 0·028 sq. in.
Total area of metal = 0·0302 sq. in.

Specimen.	Breaking load.	Breaking stress.
	tons.	tons per sq. inch.
1	3·53	117·0
2	3·70	122·6
	Mean 3·615	Mean 119·8

The authors desire to express their thanks to Prof. D. A. Low for his valuable supervision and suggestions.

THE LAWS OF AVANZINI:

LAWS OF PLATES MOVING AT AN ANGLE IN FLUIDS

BY LT.-COL. R. DE VILLAMIL (R.E., retired)

FOREWORD

If one reflects on the great army of scientific men who are at present studying the subject of the motion of planes, at an angle, in fluids, it is very remarkable that the work of Avanzini on this subject should have escaped notice. All that appears to be known about this diligent experimentalist is in reference to what is known as "Avanzini's law," in relation to the position of the centre of pressure on a moving plate. This law is quoted by Professor Bryan and M. Alexandre Sée (to quote two only of the most eminent writers on the subject), but they omit to say from whence they get the formula. M. Alexandre Sée, in a private letter, has kindly informed me that "je crois qu'on a appelé 'Loi d'Avanzini' *une loi qu'il n'a jamais formulée lui même*, mais qu'il a étudiée." Whatever Avanzini may have said on the subject is probably to be found in the *Memorie della Academia di Padova*, but the volume containing his memoir does not appear to be in England; in any case I have not been able to find it.

Avanzini was a priest and a Professor of Mathematics in the North of Italy. The work of his to which I specially refer here was published in Bologna in the *Memorie dell' Istituto Nazionale Italiano*, in the first decade of the nineteenth century. These volumes are very rare, but by the courtesy of the Royal Society I have been enabled to give copies of the tables and plates in the following pages.

Avanzini started from the principle that there are six variables in the problem: (1) length of plate, (2) breadth of plate, (3) velocity of plate in still water, (4) angle of attack, (5) position of the centre of pressure of the fluid, and (6) the density of the fluid. In each of his series of experiments he keeps four of these constant and then studies the relation between the other two. The experiments were very carefully arranged and carried out, and the results obtained are always consistent with one another; they are therefore well worth rescuing from oblivion; even though one may not agree with all his deductions and explanations.

If we compare them with Langley's* we see how much more reliable they appear to be. For example, if we refer to the chapter on "The counterpoised eccentric plane" in the latter work we see that in some of the experiments the angle of attack varied as much as 82°, 80°, 72° and 68°; that is to say 41°, 40°, 36° and 34° on either side of the *mean* angle. Under these circumstances it is impossible to attach any very great importance to the exact value of the "mean angle"; more especially as, when the axis of the plate is fixed centrally, the angle of attack was found to vary from 78° to 90°. Langley, referring to his Table XVII. on this subject, says:—"It will be noticed that this angle $(90° - \theta)$ is 5°·5 for the case when the axis of rotation passes through the centre of the plane—a setting for which the plane must be vertical. This observed angle of 5°·5 is to be explained, not by a tipping of the plane, but by a tipping of the line of reference due to a yielding of the supports, &c., to the wind of rotation. This angular deflection, therefore, becomes a correction to be applied to all the observations." This would appear to be a very rough and ready method of employing an "index error" *which, presumably, varies with the velocity.* Avanzini tells us his plates did not oscillate, visibly, and that the differences in the angles observed did not exceed a few minutes of a degree (*pochi minuti*); there was also no index error. In Langley's experiments the oscillations of the plate appear to have been so great as to completely mask Avanzini's second law—that is that

$$\theta \propto \frac{1}{f(V)},$$

where θ = angle of attack, and V = velocity of the plate.

In studying the subject it is, perhaps, always best to start with experiments in water; they are more reliable, being subject to fewer disturbances. The experiments here quoted were, however, made in both fluids.

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* Experiments in Aerodynamics, 1891.

The velocities employed by Avanzini were rather low; and it may be contended that these laws *may not be equally true for higher speeds.* Whether this is so, or not, is for experiment to decide; but there is no apparent reason why there should be a change of law at higher velocities (*which are not excessive*).

As regards the measures of length, some trouble has been experienced in trying to find out what is the size of the *piede* (which has been translated as *foot*). Colonel Duchemin, writing about Avanzini's work, says he employed the "Turin foot," which he says was = $O_m.5136524$, and was divided into 12 inches (*pollici*); 1 inch = 12 lines (*linee*), 1 line = 12 points. The Turin foot is sometimes called the "Piedmont foot" or the "Sardinian foot" (*Piede liprando*), and is stated in tables to be = 20·22 English inches.

If Avanzini employed this *piede* (and there is reason to believe it was the "scientific measure" in the North of Italy at this time) then the velocities were more than 60 per cent. greater than they would appear to be at first sight. The exact value is of no special importance here as the laws enunciated, are qualitative only.

Law 1

WHEN a rectangular lamina is immersed in a liquid, at rest (except when its surface is horizontal) the centre of pressure of the liquid on the plate will fall between the centre of figure and the lower edge of the plate: this is a natural consequence of the pressure increasing with the depth of immersion.

When, however, the lamina moves in the liquid, all this is changed. If the angle of attack is a right angle, the centre of pressure will be found to be at the centre of figure of the plate; whilst if the angle of attack is acute the centre of pressure will be *between the centre of figure and the leading edge*—whether the leading edge be horizontal and above, or below, the trailing edge, or whether the leading edge be vertical.

There is no reason for believing that Newton was aware of this fact; for, even after his time, that great quartet, Euler, Bernouilli, S'Gravesande and d'Alembert, made no mention of it. The last of these even says "it must be observed that the centre of pressure of the fluid is, or must be assumed to be at the centre of gravity of the figure.†

Even at the close of the eighteenth century (1798) neither Vince, nor that very close observer Dubuat, make any reference to it.‡

Very early in the nineteenth century Avanzini carried out a series of remarkable experiments, from which he deduced the laws of the aeroplane. His paper was presented to the *Istituto Nazionale Italiano* on the 2nd July, 1804, and was published in the first part of the first volume of the Memoirs in 1806. These laws I have, therefore, called the "Laws of Avanzini," as he appears to be the first to have pointed them out; and to prevent any misunderstanding I define a law of Nature as an invariable sequence. To give an example: if a stone is dropped it *invariably* falls to the ground; this one may call the *qualitative* law of gravitation. Avanzini's laws, which are very imperfectly known, even at the present day, are only *qualitative* laws; the *quantitative* laws are still very obscure. I might add, further, that even the name of Avanzini is hardly remembered.

Description of Apparatus

The apparatus employed in carrying out the experiments is very fully shown in the accompanying Plates I. and II., which are photographic reproductions of the plates in the Memoirs of the *Istituto Nazionale Italiano.*

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† Il faut observer que le centre de pression du Fluide est ou doit être censé au centre de gravité de la Figure. (*Essai d'une nouvelle Théorie de la résistance des fluides.*)

‡ It appears doubtful if, in 1889—nearly another century later—Lilienthal was not ignorant of this; though Colonel Duchemin in 1842 had gone into the question very fully.

Fig. 1, Table II., is a general view of the railway, showing the carriage with an experimental plate held in position. The track was about 140 feet long and the carriage was moved along it by means of falling weights, as shown in Fig. 1, an arrangement which needs no explanation. To allow for the diminished resistance of the cord, as it was wound up, part of the accelerating weights were chains, Fig. 12, which, by resting on the ground, gradually reduced the *effective* weights; a very uniform motion of the carriage was thus obtained. Lights were fixed at every ten of the last seventy feet of the run, OO, Fig. 1, and trained observers recorded the time taken by the plate in moving past each ten feet. The motion was found to be perfectly uniform; and two heavy balls suspended by cords, Q Q, Fig. 1, acted as buffers in checking the motion of the carriage at the end of the run.

Fig. 2 gives an enlarged view of the carriage, which had brass wheels and side rollers, as shown. Figs. 3 and 4 are still more enlarged views of the Y-shaped plate holder, showing how it was fixed to the carriage. It will be seen that the leading edge of the plate could be fixed horizontally, vertically, or at any required angle. Between the forks of the Y-piece, the small plate with semi-circular ears (Fig. 6) was placed and held in position by the small screws W W, Fig. 4. This plate is shown in position in Fig. 9.

Since the plate was free to turn about the pivots and it was necessary to measure the angle at which it was moving, the arrangement K K, Fig. 1—shown in detail in Fig. 14—was adopted. The sharp point O could be adjusted as desired, so that the plate should strike it and be marked by it. The measurement of the angle was then made as shown in Figs. 3 and 11, Table I.; where angles are shown as being measured both vertically and horizontally.

Fig. 15, Table I, is a representation of the brick canal (150 feet long) which could be filled, or not, with water; and in which the plate moved. It will be evident that this arrangement was very superior to any system of whirling table, since all centrifugal action was absent, and there were not any eddies produced by the apparatus itself. Also when the plates were moving in the air, they were sheltered from any disturbing winds.

Since it was necessary to be quite sure that there was no disturbance of the fluid caused by " boundaries "—in order that the fluid might be considered to be of " indefinite " extent—Avanzini had small pith balls, fastened by short pieces of fine silk, on the inside of the canal, for the experiments in water. In all cases, whatever the velocity or the angle of inclination of the plates, it was found that no movement of these pith balls could be observed. Similarly, when the canal was empty, he strewed very light feathers on the bottom; the moving plates causing no visible disturbance of these. It was evident, therefore, that there were no perceptible eddies in the fluids resulting from the boundaries, and that the fluids might consequently be considered as being " indefinite."

All being ready the experimental plate was fixed to the small plate, Fig. 6 (Tav. II.), and the carriage was released.

During the first twenty feet or so, the plate oscillated; but when the motion of the carriage became uniform the plate came to rest at a fixed angle and remained at this angle during the travel of considerably more than the last seventy feet. The angle was then measured.

As Avanzini says:—

" 1°. To cause the lamina to move normally to the direction of its motion, it is necessary that the axis of the pivots, or of its own equilibrium, should pass through the centre of figure; 2°, to move it at a given angle to the direction of its own motion it is necessary that the aforesaid axis of equilibrium should be between the leading edge of the lamina and the centre of figure, and at a greater or less distance from this centre according to the smaller or greater inclination that it is desired the lamina should assume." It was to prove this that the first series of experiments was carried out.

The sizes and weights of the plates experimented with are given in the following table:—

WATER.

Lamina.	Length.	Breadth.	Weight.
I.	9 inches	6 inches	16 ounces
II.	9 ,,	4 ,,	15 ,,
III.	9 ,,	3 ,,	13 ,,
IV.	6 ,,	4 ,,	7 ,,
V.	6 ,,	3 ,,	6 ,,
VI.	6 ,,	2 ,,	4 ,,

AIR.

Lamina.	Length.	Breadth.	Weight.
I.	18 inches	4 inches	5 ounces
II.	13 in. 6 lines	4 ,,	4 ,,
III.	9 inches	4 ,,	$2\frac{1}{2}$,,
IV.	9 ,,	6 ,,	4 ,,
V.	9 ,,	8 ,,	5 ,,

12 lines = 1 inch; 12 inches = 1 foot (*piede*).

Weights are in Italian ounces.

If the foot was the *piede liprando*, of over 20 English inches in length, it is evident that the "inches" (*pollici*) were about 1·6 English inches in length, the "lines" being one-twelfth of this.

For the experiments in water the plates were of iron, smooth and flat and of the thickness of about half a line—one-twentyfourth of an inch, since 12 lines = one inch. For the experiments in air the plates were of wood about one line (one-twelfth of an inch) in thickness.

In the following tables the "Distance of the axis of equilibrium" from the "centre of figure" of the plate is always expressed in *twentyfourths of the length of the lamina—or twelfths of the half length.* The "angle" of the plate is the angle, on a vertical section (if the upper edge of the plate is horizontal) between the face of the plate and the line of motion; this is what the French call the "angle of attack."

Avanzini says he "thinks it important to state:—1°. that each of these experiments is the mean result of twenty or more which were made; 2°. that the maximum difference between these latter, in the measurement of the angles, did not exceed a few minutes of a degree."

In order to balance the plates when the axis of support did not pass through the centre of figure, it was necessary to cause the centre of gravity to pass through this axis. To do this small holes were bored in the plates, and these were filled with small heavy bodies in the case of the wooden plates, and with light bodies in the case of the iron plates. By this means a proper balance was obtained and the action of gravity was eliminated; the plates could then, when moving steadily, be considered as having no *weight.*

In the problem to be solved there are six variables :—

1. Length of plate.
2. Breadth of plate.
3. Velocity of plate.
4. Angle of inclination of plate (angle of attack).
5. Position of axis of equilibrium when the motion is "steady."
6. Density of the fluid.

By keeping any four of these constant it is easy to see how the remaining two depend on one another. It will also be obvious that if the plate, which is free to turn about the axis of suspension, moves steadily at a constant angle, the centre of pressure must pass through this axis of suspension ; if it were situated anywhere else a couple would be formed and the plate would rotate about the axis.

The first series of experiments, given below, were carried out to show where the axis of equilibrium was situated.

Position of axis of equilibrium

In the following experiments attention was specially directed towards the relation between the "angle of attack" and the position of the axis of equilibrium. The first fourteen series were conducted in water and the remaining ten in air. Viewed generally, the results were the same in both cases.

EXPERIMENTS CONDUCTED IN WATER

Experiment I

Lamina I. Velocity = 1 ft. in 0·60″

Dist. of axis ..	0	1	2	3	4
Angle	90°	53°	34°	25° 40′	21° 30′

Experiment II

Lamina I. Velocity = 1 ft. in 0·30″

Dist. of axis	0	3	4
Angle	90°	15° 40′	12° 25′

Experiment III

Lamina II. Velocity = 1 ft. in 0·60″

Dist. of axis	0	3	4
Angle	90°	25°	21°

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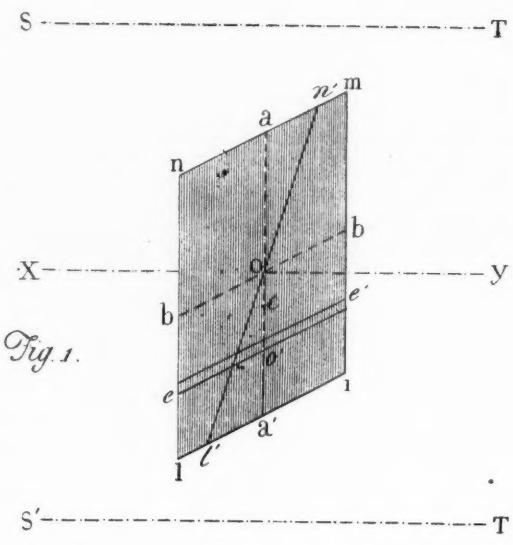


Fig. 1.

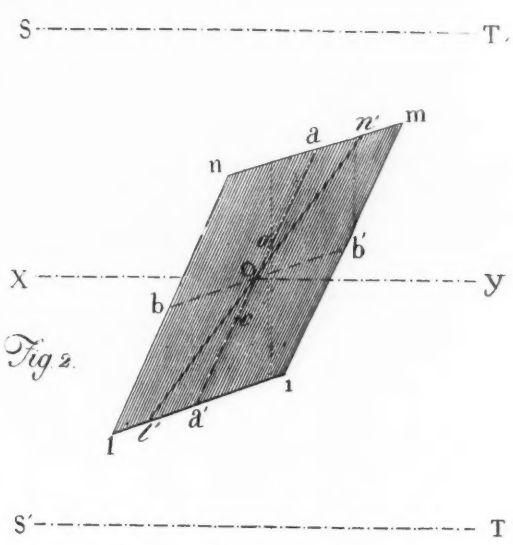
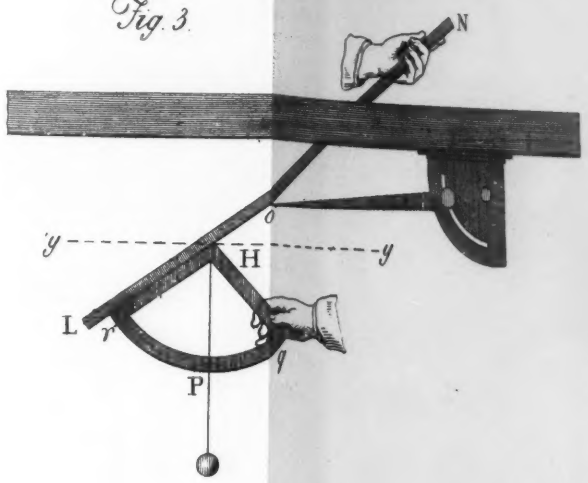


Fig 2.

Fig. 3.



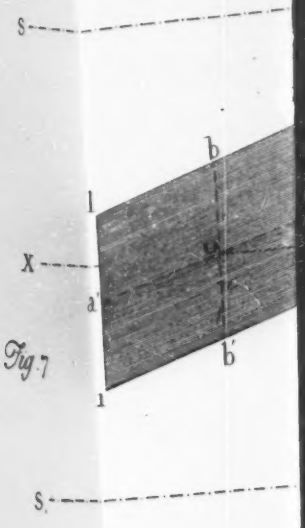
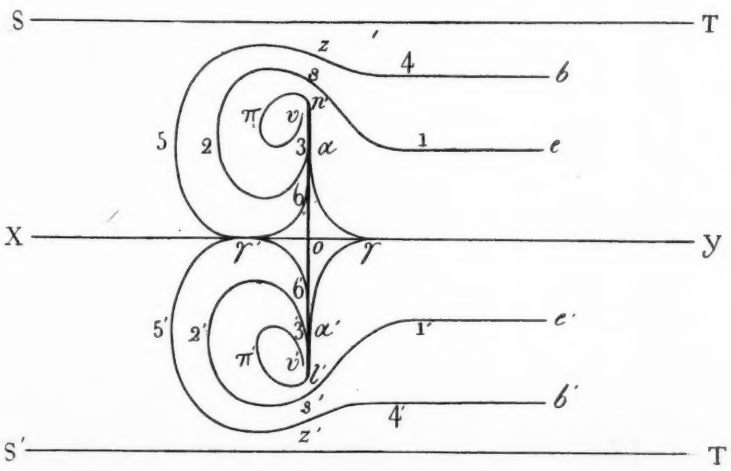


Fig. 7



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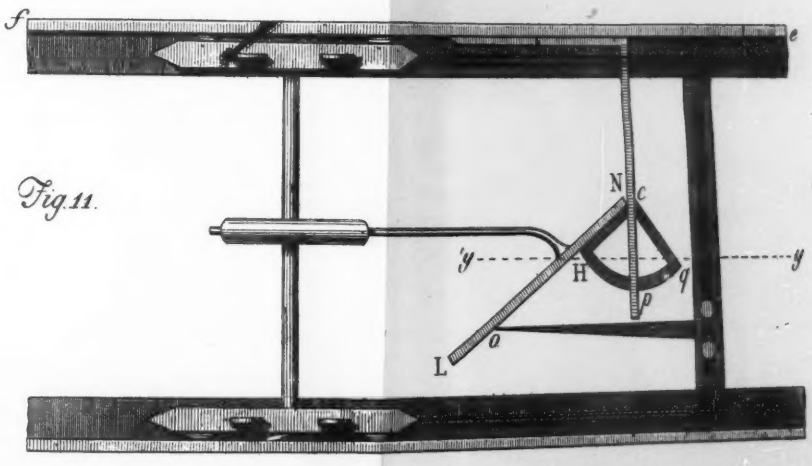
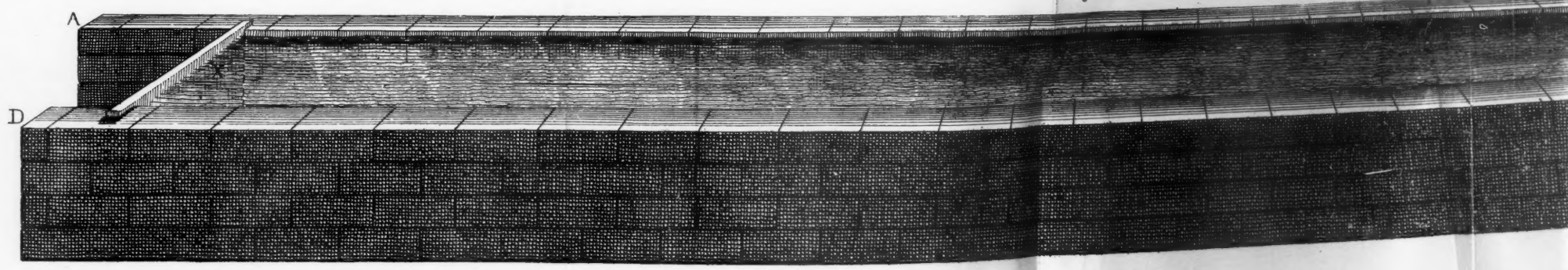
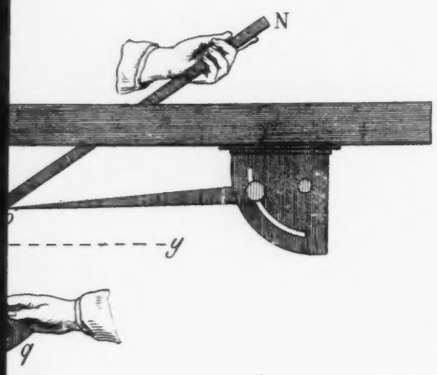


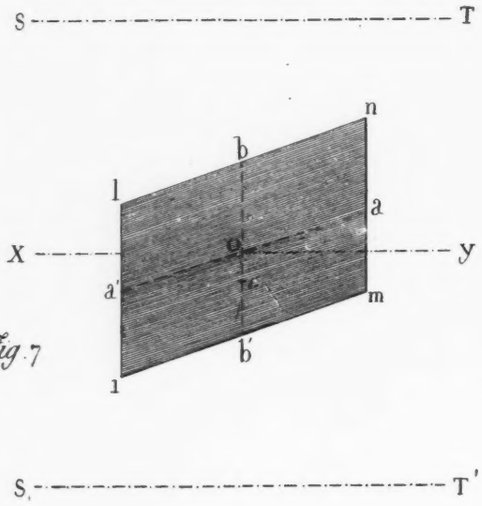
Fig. 11.

Fig 6

Fig 15.







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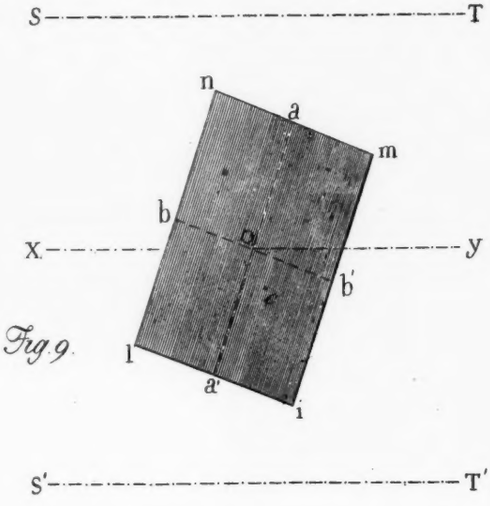

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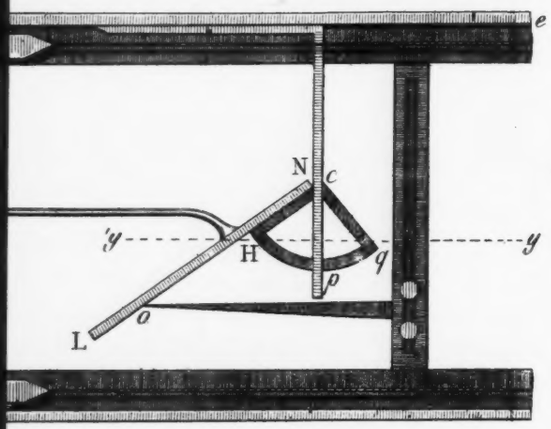

Fig 6

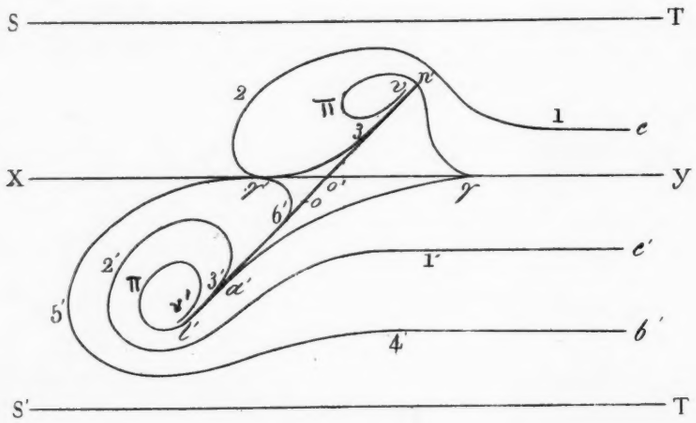

Fig 15

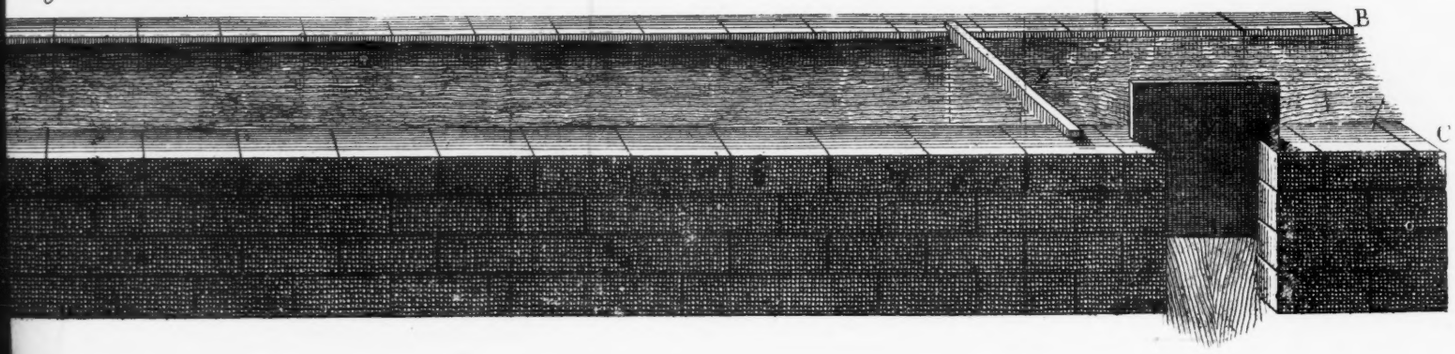

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Fig. 13

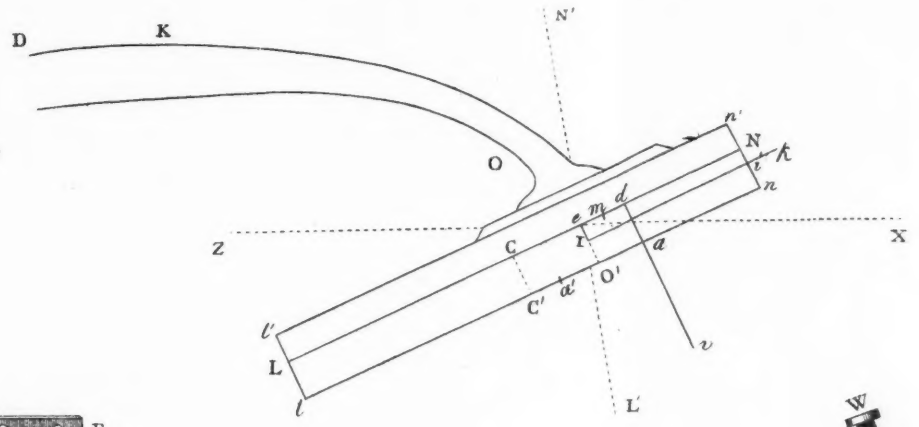


Fig. 8

Fig. 7.

Fig. 5.

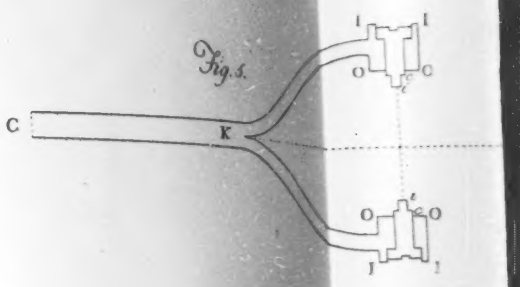
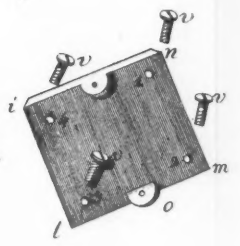


Fig. 6.



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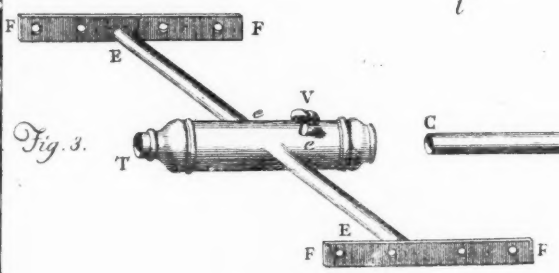

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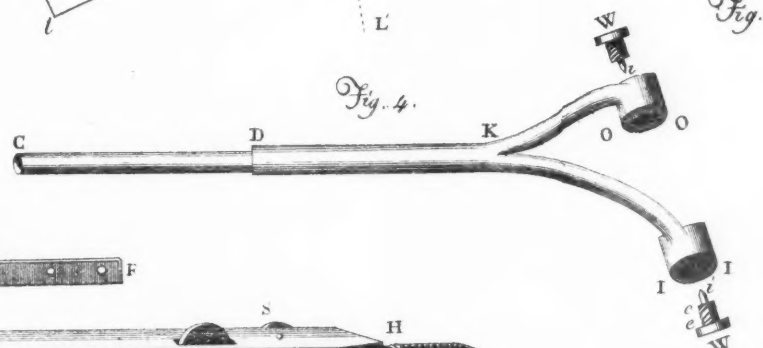


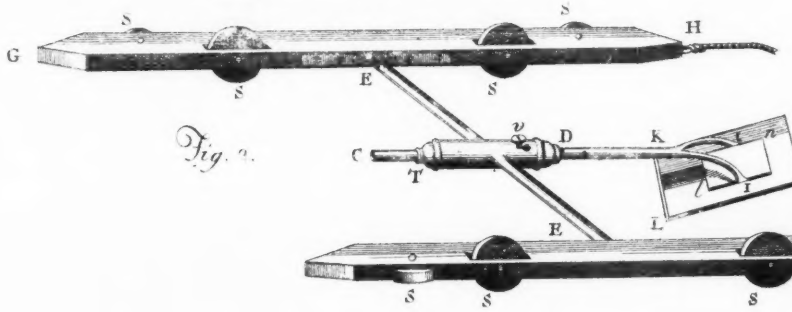
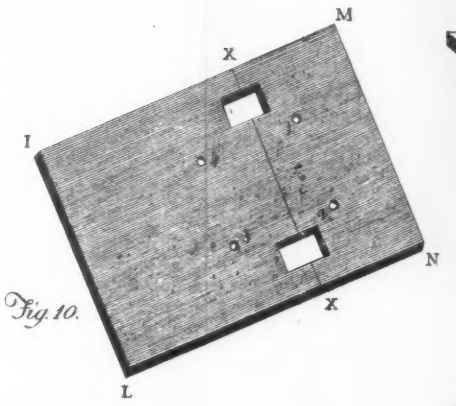


Fig. 2.

Fig. 10.



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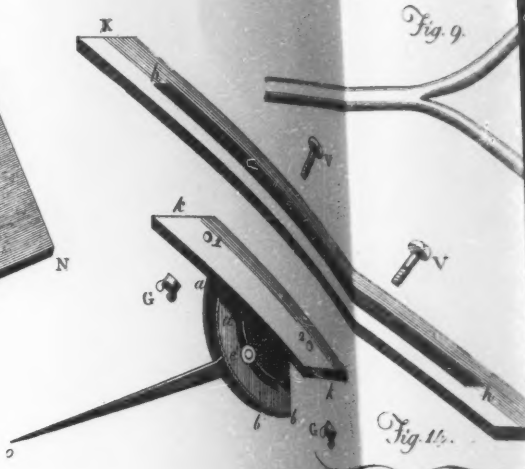

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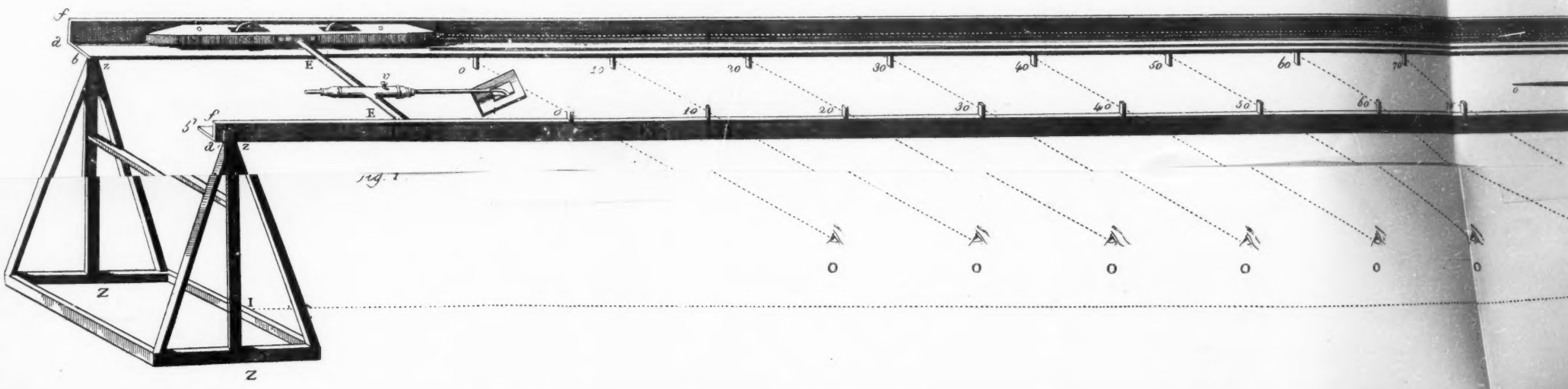


Fig. 1.

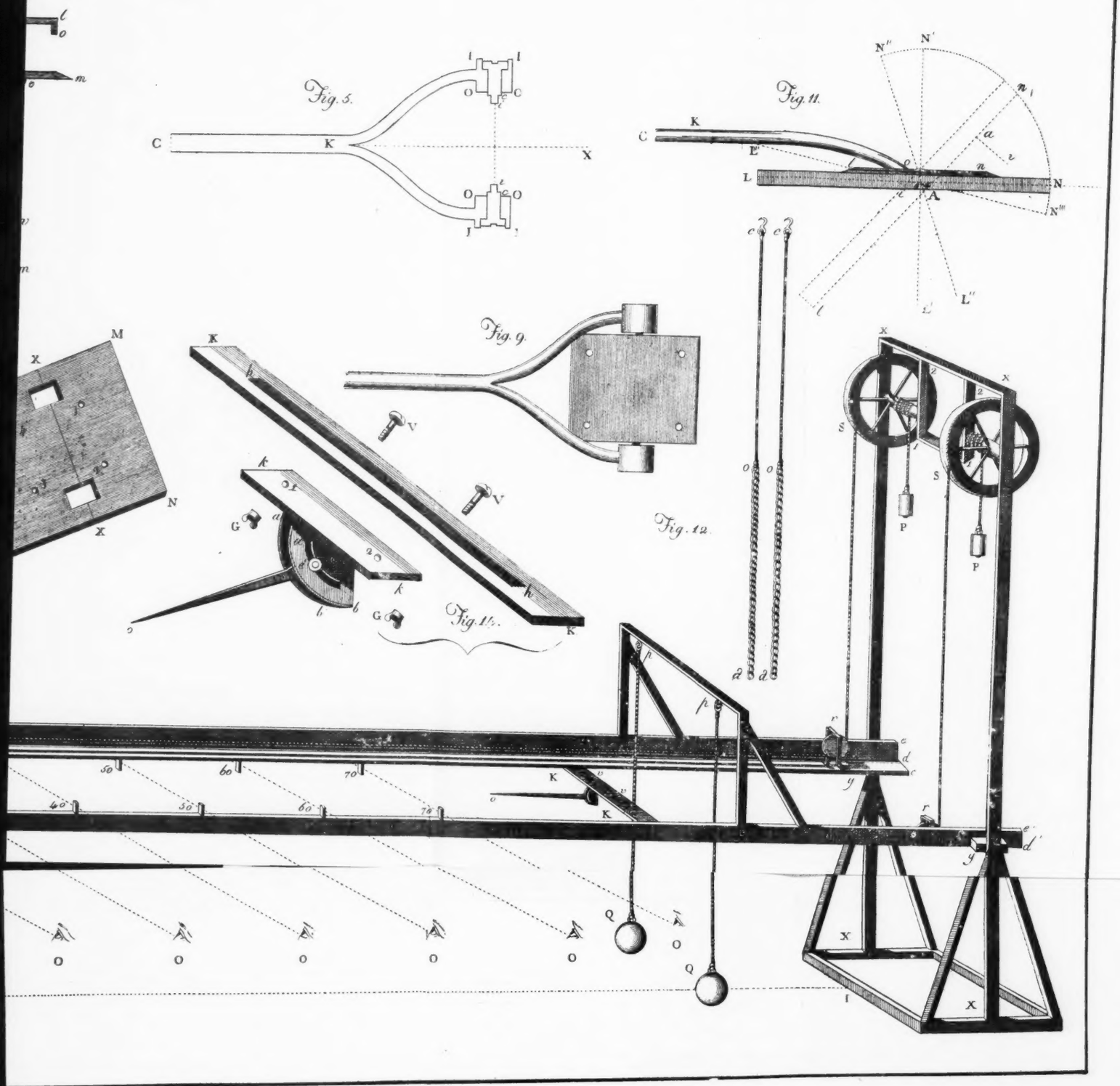


Fig. 5.
Fig. 11.
Fig. 9.
Fig. 12.
Fig. 13.

EXPERIMENT IV

Lamina II. Velocity = 1 ft. in 0·47″

Dist. of axis	0	4	
Angle	90°	8° 30′	

EXPERIMENT V

Lamina II. Velocity = 1 ft. in 0·30″

Dist. of axis	0	3	4
Angle	90°	13° 20′	12°

EXPERIMENT VI

Lamina II. Velocity = 1 ft. in 0·23″

Dist. of axis	0	4	
Angle	90°	10° 45′	

EXPERIMENT VII

Lamina III. Velocity = 1 ft. in 0·60″

Distance of the axis of equilibrium	0	3	4
Angle	90°	24° 15′	20° 25′

EXPERIMENT VIII

Lamina III. Velocity = 1 ft. in 0·47″

Dist of axis	0	4	
Angle	90°	17° 35′	

EXPERIMENT IX

Lamina IV. Velocity = 1 ft. in 0·60″

Dist. of axis	0	1	2	3	4
Angle	90°	54°	34° 30′	25° 30′	21° 30′

EXPERIMENT X

Lamina IV. Velocity = 1 ft. in 0·38″

Dist. of axis	0	4	
Angle	90°	15°	

EXPERIMENT XI

Lamina IV. Velocity = 1 ft. in 0·19″

Dist. of axis	0	4	
Angle	90°	8° 44′	

EXPERIMENT XII

Lamina V. Velocity = 1 ft. in 0·60″

Dist. of axis	0	2	
Angle	90°	34°	

EXPERIMENT XIII

Lamina VI. Velocity = 1 ft. in 0·60″

Dist. of axis	0	2	
Angle	90°	33° 25′	

EXPERIMENT XIV

Lamina VI. Velocity = 1 ft. in 0·38″

Dist. of axis	0	2	
Angle	90°	27° 50′	

EXPERIMENTS CONDUCTED IN AIR

EXPERIMENT I

Lamina I. Velocity = 1 ft. in 0·15″

Dist. of axis	0	4	5
Angle	90°	13° 56′	6° 37′

EXPERIMENT II

Lamina I. Velocity = 1 ft. in 0·30″

Dist. of axis	0	4	5
Angle	90°	15° 33′	9° 25′

EXPERIMENT III

Lamina II. Velocity = 1 ft. in 0·15″

Dist. of axis ..	0	3	4	5
Angle	90°	37° 30′	19° 15′	7° 46′

EXPERIMENT IV

Lamina II. Velocity = 1 ft. in 0·30″

Dist. of axis	0	4	5
Angle	90°	28° 38′	9° 30′

EXPERIMENT V

Lamina III. Velocity = 1 ft. in 0·15″

Dist. of axis	0	4	5
Angle	90°	26° 6′	12° 26′

EXPERIMENT VI

Lamina III. Velocity = 1 ft. in 0·30″

Dist. of axis ..	0	3	4	5
Angle	90°	52° 22′	27° 41′	15° 20′

EXPERIMENT VII

Lamina IV. Velocity = 1 ft. in 0·15″

Dist. of axis	0	4	5
Angle	90°	27° 39′	15° 36′

EXPERIMENT VIII

Lamina IV. Velocity = 1 ft. in 0·30″

Dist. of axis ..	0	3	4	5
Velocity	90°	49° 50′	28° 30′	17° 50′

EXPERIMENT IX

Lamina V. Velocity = 1 ft. in 0·15″

Dist. of axis	0	4	5
Angle	90°	28° 9′	18° 40′

EXPERIMENT X

Lamina V. Velocity = 1 ft. in 0·30″

Dist. of axis	0	4	5
Angle	90°	29° 15′	20° 35′

"From the foregoing tables it appears evident that in both of the fluids, and at all velocities and for all sizes of the plate, the angle under which it comes to rest is always a right angle when the axis passes through the centre of figure; and always acute when this axis passes between the centre of figure and the superior edge of the lamina, becoming acuter as the distance of the axis from the centre of figure increases."

"The differences between the *results of the experiments* on the plates of the indicated dimensions and the *results deduced from theory* are, as will be seen, *so great* that . . . I have thought it useless to renew the experiments with lamina of greater size."

The words italicized show that the laws deduced from the experiments were not known at the time Avanzini wrote this.

Avanzini's First Law

When the "angle of attack" of a small rectangular lamina, moving steadily in a liquid, is a right angle, the centre of pressure passes through the centre of figure; when, however, the angle of attack is acute, this centre of pressure is situated between the centre of figure and the leading edge, and approaches this leading edge as the angle of attack becomes acuter. This is equally true for both water and air.

Laws 2 and 3

Although it would not be very difficult to trace the relations between the other variables of the problem, from the foregoing experiments, nevertheless Avanzini thought it "would not be superfluous to represent in several tables these same variations."

The next series of experiments were conducted with a view to showing the relation between the velocity of the plate and the angle of attack. The apparatus was the same and the results are given in the following tables:—

ANGLES CORRESPONDING TO VARIOUS VELOCITIES

In Water

Experiment I

Lamina II. Dist. of axis of equilibrium = 4

Velocity	1 ft. in 0·30″	1 ft. in 0·47″	1 ft. in 0·60″
Angle	12°	18° 30′	21°

Experiment II

Lamina IV. Dist. of axis = 4

Velocity	1 ft. in 0·38″	1 ft. in 0·60″	
Angle	15°	21° 30′	

Experiment III

Lamina I. Dist. of axis = 4.

Velocity	1 ft. in 0·30″	1 ft. in 0·60″	
Angle	12° 25′	21° 30′	

Experiment IV

Lamina II. Dist. of axis = 4.

Velocity	1 ft. in 0·30″	1 ft. in 0·60″	
Angle	13° 20′	25°	

Experiment V

Lamina III. Dist. of axis = 3.

Velocity	1 ft. in 0·47″	1 ft. in 0·60″	
Angle	17° 35′	20° 25′	

EXPERIMENTS CONDUCTED IN AIR

Experiment I

Lamina I. Dist. of axis = 4.

Velocity	1 ft. in 0·15″	1 ft. in 0·30″	
Angle	13° 56′	15° 33′ (*sic*)	

EXPERIMENT II

Lamina I. Dist. of axis = 5.

Velocity	1 ft. in 0·15″	1 ft. in 0·30″	
Angle	6° 37′	9° 25′	

EXPERIMENT III

Lamina II. Dist. of axis = 4.

Velocity	1 ft. in 0·15″	1 ft. in 0·30″	
Angle	19° 15′	20° 38′	

EXPERIMENT IV

Lamina II. Dist. of axis = 5.

Velocity	1 ft. in 0·15″	1 ft. in 0·30″	
Angle	7° 46′	9° 30′ (*sic*)	

EXPERIMENT V

Lamina III. Dist. of axis = 4.

Velocity	1 ft. in 0·15″	1 ft. in 0·30″	
Angle	26° 6′	27° 41′	

EXPERIMENT VI

Lamina III. Dist. of axis = 5.

Velocity	1 ft. in 0·15″	1 ft. in 0·30″	
Angle	12° 26′	15° 20′	

EXPERIMENT VII

Lamina IV. Dist. of axis = 4.

Velocity	1 ft. in 0·15″	1 ft. in 0·30″	
Angle	27° 39′	28° 30′	

EXPERIMENT VIII

Lamina IV. Dist. of axis = 5.

Velocity	1 ft. in 0·15″	1 ft. in 0·30″	
Angle	15° 36′	17° 50′	

EXPERIMENT IX

Lamina V Dist. of axis = 4.

Velocity	1 ft. in 0·15″	1 ft. in 0·30″	
Angle	28° 9′	29° 15′	

EXPERIMENT X

Lamina V. Dist. of axis = 5.

Velocity	1 ft. in 0·15″	1 ft. in 0·30″	
Angle	18° 40′	20° 35′	

In reference to these Avanzini says :—" From the preceding tables it becomes manifest that the acute angle, at which a given lamina comes to rest, whatever may be its size or the distance of the axis of equilibrium, and the fluid in which it is moving, becomes smaller as the velocity of the lamina increases."

" From that follows the necessary consequence that the centre of resistance encountered by a thin and smooth rectangular body moving in water, or in air, at rest, at any angle acute to the direction of its motion, will be *less distant from the centre of figure* of the anterior surface of the solid, *as the velocity increases.*"*—(Avanzini.)

I have been careful to give Avanzini's own words because I disagree with his " necessary consequence " ; and I have italicised the words to which I wish to draw particular attention.

If I read his meaning correctly, it is that *as the velocity increases* the centre of pressure on the *leading surface* of the plate *shifts* gradually *towards the centre of figure* and so tends to cause the plate to rotate about the axis of support and by so doing *reduces the angle of attack.*

The explanation appears plausible since, (1) the plate undoubtedly rotates about this axis of support, and (2) there must be a couple produced to cause this rotation.

We know, however, by Avanzini's first law, that the centre of pressure *approaches the leading edge of the lamina as the angle of attack becomes acuter.* We should therefore have a centre of pressure which is getting " less distant from the centre of figure," whilst it is also " approaching the leading edge, as the angle of attack becomes more acute "—a contradiction which it is impossible to accept. The explanation must be sought for elsewhere.

* Da ciò segue di necessaria consequenza che il centro di resistenza incontrata da un piano e sottil corpo rettangolare moventesi per l'acqua o per l'aria tranquilla sotto un qualunque angolo acuto colla direzione del suo movimento, sarà *tanto meno distante dal centro di grandezza della superficie anteriore* del solido, quanto *piu grande* sarà *la sua velocita.*

The pressure on the plate may be considered as being divided into two parts: the pressure on the anterior surface and that on the posterior surface; that on the latter being less than that on the former. For simplicity I will call the pressure on the anterior surface *positive* and that on the posterior surface *negative*. Now, referring to Fig. 6, Table I., let us suppose the plate l^1n^1 to be moving steadily at a fixed angle and that o is the centre of figure, whilst o^1 is the axis of equilibrium. It is manifest that, since there is equilibrium, the centre of pressure of the *positive* pressure must pass through the point o^1; also the centre of pressure of the *negative* pressure must equally pass through o^1. The moment of the pressure on o^1l^1 = the moment of the pressure on o^1n^1. Since the plate is completely immersed in the fluid, the point of greatest *positive* pressure must be at o^1, and therefore one may call the point o^1 the "Divide"—the point from which the fluid flows upwards and downwards past the edges of the plate; more fluid will, of course, flow past l^1 than past n^1. Similarly, the point of least *negative* pressure—measured negatively, of course—must also be at o^1.

If the velocity of the lamina be increased there appears to be no reason why the position of the "Divide" should be altered—all the reasons would appear to point to the reverse. There are, however, very strong reasons for believing that the centre of pressure of the *negative* pressure will be shifted from o^1 towards n^1. This by reducing the pressure—*measured positively*—on the posterior surface of l^1o^1 will produce a *couple* which will cause the plate to rotate, clockwise, in Fig. 6. In other words, the centre of pressure (measured positively) on the posterior surface will be shifted from o^1 towards the leading edge, n^1, whilst the position of the "Divide" will not change. When the plate has revolved through a small angle and the *proper* "angle of attack" has been reached, the centre of *negative* pressure will once more return to o^1 and remain there. In other words, the centre of the ring vortex formed behind the plate will oscillate gently from o^1 towards n^1 and eventually back again to o^1.

Whether this explanation be considered satisfactory or not, I prefer to state Avanzini's second law without his "necessary consequence."

Avanzini's Second Law

When the axis of equilibrium is at a fixed distance from the centre of figure, the velocity and the angle of attack alone varying, as the velocity *increases* the "angle of attack" *decreases*.

In other words, the angle of attack varies as an inverse function of the velocity, or, in algebraical shorthand:—

$$\theta \propto f\left(\frac{1}{V}\right)$$

where θ = angle of attack, and
V = velocity.

The next series of experiments were carried out to show the relation that exists between the length of the plate and the angle of attack.

ANGLES CORRESPONDING TO VARIOUS LENGTHS

WATER

EXPERIMENT I

Breadth of Lamina = 4 ins. Dist. of axis = 3.

Velocity = 1 ft. in 0·60″

Length	9 inches	6 inches	
Angle	25°	25° 30′	

EXPERIMENT II

Breadth of Lamina = 4 ins. Dist. of axis = 4.
Velocity = 1 ft. in 0·47″

Length	9 inches	6 inches	
Angle	18° 30′	19°	

EXPERIMENT III

Breadth of Lamina = 4 ins. Dist. of axis = 4.
Velocity = 1 ft. in 0·19″

Length	9 inches	6 inches	
Angle	8°	8° 4′	

ANGLES CORRESPONDING TO VARIOUS LENGTHS

AIR

EXPERIMENT I

Breadth of Lamina = 4 ins. Dist. of axis = 4.
Velocity = 1 ft. in 0·15″

Length	18 inches	13½ inches	9 inches
Angle	13° 56′	19° 15′	26° 6′

EXPERIMENT II

Breadth of Lamina = 4 ins. Dist. of axis = 4.
Velocity = 1 ft. in 0·30″

Length	18 inches	13½ inches	9 inches
Angle	15° 33′	20° 38′	27° 4′

EXPERIMENT III

Breadth of Lamina = 4 ins. Dist. of axis = 5.
Velocity = 1 ft. in. 0·15″

Length	18 inches	13½ inches	9 inches
Angle	6° 37′	7° 46′	12° 26′

EXPERIMENT IV

Breadth of Lamina = 4 ins. Dist. of axis = 5.

Velocity = 1 ft. in 0·30″

Length	18 inches	13½ inches	9 inches
Angle	9° 25′	9° 30′	15° 20′

" These tables prove that augmenting the length of the plates decreases the angle under which they remain inclined whilst travelling the seventy feet." To this is unfortunately added :—" By the reasoning in the last section they show that the centre of resistance encountered by a thin and smooth rectangular body moving in water, or air, at rest under any acute angle will be *less distant from the centre of figure of the anterior surface of the solid*, as the longitudinal side of the same surface becomes greater."*

The argument is unsound. There is one position, and one position only, for the centre of pressure when the plate is moving without rotation, and that is through the axis of equilibrium.†

Avanzini's Third Law

If the length of the plate be increased, the breadth, velocity and distance of the axis from the centre of figure being kept constant ; as the length of the plate increases the angle of attack decreases ; or—

$$\theta \propto f\left(\frac{1}{L}\right)$$

where L = length of plate and θ = angle of attack. This is most marked at high velocities.

Laws 4 and 5

In the next series of experiments the length of the plate, velocity and distance of the axis of equilibrium were kept constant, the only variables being the breadth of the plate and the angle of attack.

ANGLES CORRESPONDING TO VARIOUS BREADTHS

WATER

EXPERIMENT I

Length of Lamina, 9 ins. Velocity = 1 ft. in 0·60″

Distance of the axis of equilibrium = 3.

Breadth	6 inches	4 inches	3 inches
Angle	25° 40′	25°	24° 15′

* Pei ragionamenti del § precedente ci manifesta che il centre di resistenza incontrata da un piano e sottil corpo rettangolare moventesi per l'acqua, e per l'aria tranquilla sotto un qualunque angolo acuto sarà tanto meno distante dal centro di grandezza della superficie anteriore del solido, quanto più sarà grande il lato longitudinale della superficie medesima.

† From what was said about the second law, the reader will easily see that the correct explanation is that *increasing the length of the plate* produces the *same effect on the vortex at the rear of the lamina*, as increasing the velocity. There appears to be no reason for supposing that the position of the " divide " is altered.

EXPERIMENT II

Length of Lamina = 9 ins. Velocity = 1 ft. in 0·60″

Distance of axis of equilibrium = 4.

Breadth	6 inches	4 inches	3 inches
Angle	21° 30′	21°	20° 25′

EXPERIMENT III

Length of Lamina = 6 ins. Velocity = 1 ft. in 0·60″

Distance of axis of equilibrium = 2.

Breadth	4 inches	3 inches	2 inches
Angle	34° 30′	34°	33° 25′

AIR

EXPERIMENT I

Length of Lamina = 9 ins. Velocity = 1 ft. in 0·15″

Distance of the axis of equilibrium = 4.

Breadth	8 inches	6 inches	4 inches
Angle	28°	27° 39′	26° 6′

EXPERIMENT II

Length of Lamina = 9 ins Velocity = 1 ft. in 0·30″

Distance of axis of equilibrium = 4.

Breadth	8 inches	6 inches	4 inches
Angle	29° 15′	28° 30′	27° 41′

EXPERIMENT III

Length of Lamina = 9 ins. Velocity = 1 ft. in 0·15″

Distance of the axis of equilibrium = 5.

Breadth	8 inches	6 inches	4 inches
Angle	18° 40′	15° 36′	12° 26′

EXPERIMENT IV

Length of Lamina = 9 ins. Velocity = 1 ft. in 0·30″

Distance of axis of equilibrium = 5.

Breadth	8 inches	6 inches	4 inches
Angle	20° 35′	17° 50′	15° 20′

"From the preceding tables it appears that increasing the breadth of the plates sensibly increases the acute angle under which they come to rest." After this follows a remark that this could only happen by the centre of pressure moving away from the centre of figure—an argument which appears unsound for the reasons previously given.*

Avanzini's Fourth Law

If the distance of the axis of suspension of the plate be kept at a fixed distance from the centre of figure; and if the length and velocity of the plate be kept constant the angle of attack will be *sensibly* increased by increasing the breadth of the plate; or—

$$\theta \propto f(b)$$

where b = breadth of plate and θ = angle of attack.

It will be observed that increasing the breadth of the plate acts in the opposite manner to increasing the length. This shows, what has been found out practically by aeroplane builders, the advantage of making the attack *in line* rather than in *column* formation.

Since increasing the breadth of the plate tends to *increase* the angle of attack *very slightly*, whilst increasing the length of the plate *diminishes* this angle of attack in a *much greater degree*, it would follow that there must be some proportion of the length to the breadth when these two effects will neutralise one another. This proportion, where the breadth much exceeds the depth, would apparently be the best *theoretical* shape for an aeroplane wing where stability is sought for. Doubtless, the aeroplane builders have, by a process of trial and error, *practically* found this proportion; nevertheless, it is very necessary, in order to co-ordinate one's ideas on the subject, to have a *theoretical* explanation of the reasons for this proportion.

The last series of experiments were conducted to show the effect of the density of the fluid on the angle of attack. Length, breadth and velocity of the lamina were kept constant; as well as the distance of the axis of equilibrium; the experiments were then made in air and water, with the following results:—

ANGLES CORRESPONDING TO THE TWO KINDS OF FLUIDS

EXPERIMENT I

Length of Lamina = 9 ins. Breadth = 4 ins.
Velocity = 1 ft. in 0·30″ Dist. of axis = 4.

Angle in water	12°
Angle in air	27° 41′

* It will be evident that, in this case, increasing the breadth of the plate tends, *sensibly* (though slightly) to shift the centre of the vortex behind the lamina, from o^1 towards l^1; after which it again returns to o^1. The rotation, in this case, being *counter-clockwise* in Fig. 6.

EXPERIMENT II

Length of Lamina = 9 ins. Breadth = 6 ins.
Velocity = 1 ft. in 0·30″ Dist. of axis = 4.

Angle in water	12° 25′
Angle in air	28° 30′

EXPERIMENT III

Length of Lamina = 9 ins. Breadth = 6 ins.
Velocity = 1 ft. in 0·30″ Dist. of axis = 3.

Angle in water	25° 40′
Angle in air	49° 50′

EXPERIMENT IV

Length of Lamina = 9 ins. Breadth = 4 ins.
Velocity = 1 ft. in 0·30″ Dist. of axis = 3.

Angle of water	13° 20′
Angle of air	52° 22′

"These tables show that the angles under which the plates come to rest were more acute when they were moving in water than when they were travelling in air."

To this remark is added :—"This evidently proves, by the previous reasoning, that the centre of resistance of water moves less than the centre of resistance of the air from the centre of figure of the lamina." This is a *non-sequitur*, the centre of resistance in both cases passing through the axis of equilibrium.

Avanzini's Fifth Law

Length, breadth and velocity of lamina being constant, as well as the distance of the axis of equilibrium, the angle of attack varies inversely as some function of the density of the fluid ; or—

$$\theta \propto f\left(\frac{1}{\rho}\right)$$

where ρ = density and θ = angle of attack.

From these five laws we may deduce, generally, that—

$$\theta \propto \frac{f(b)}{f(\rho) \times f(L) \times f(V)}$$

It is for experiment to determine what form these *functions* take. Up to the present there is not sufficient information to say what they are ; Avanzini's experiments only give 2 or 3 (very rarely 4) points on the curves, which are quite insufficient data. If these experiments were repeated, with the very superior appliances for recording results now available, much very valuable information would be obtained. Really useful curves could be drawn and the correct forms of the functions obtained. When this has been done it will be possible to give the *quantitative* laws of the aeroplane in probably as simple form as those *qualitative* ones of Avanzini which have just been explained. All whirling table experiments are open to suspicion, for many obvious reasons.

A careful study of these laws would prevent anyone accepting certain formulæ which have been proposed for finding the position of the centre of pressure on a rectangular plate moving at an acute angle in a fluid. For example, M. Alexandre Sée ("Les lois expérimentales de l'aviation") quotes Avanzini as having proposed the formula,

$$\frac{y}{2h} = 0{\cdot}3\ (1 - \sin i)$$

where y = distance of C. of P. from C. of F.
h = half-length of the plate.
ı = angle of attack.

This formula violates Law II.: for since the angle of attack varies *inversely* as some function of the velocity, *when the distance between the centre of pressure and the centre of figure is constant*, it follows that the *position of the centre of pressure* might be the same *for a variety of angles of attack.**

Jöessel (Alexandre Sée, "Lois expérimentales," &c.) proposed a slight variation on this, viz. :—

$$\frac{y}{2h} = 0{\cdot}305\ (1 - \sin i)$$

which is open to the same objection.

Both these formulæ, further, violate Law V., where angle of attack varies *inversely* as some function of the *density* of the fluid, *when the position of the centre of pressure remains constant*. Equally the *breadth* of the plate does not enter into the formula; and this, though no doubt not *very important*, should be taken into account..

There are strong reasons for believing that the formula is more complicated than either of these, and (2) that y becomes a *maximum*, afterwards decreasing a little.

To assist in understanding the subject it is advisable to study the diagrams of the vortices given in Figs. 5 and 6, Tav. I., of Avanzini's *Memoria*. These were drawn from theoretical deductions and were afterwards found to be correct from experimental observations, made by means of small threads of silk, as well as by the use of small balls of the same density as the liquid and which moved past the plate. Referring to Fig. 5, which shows the vortex formed when the angle of attack is a right angle, this form of vortex is not stable when the motion of the plate is being accelerated—which is, of course, the case when the motion is kept *uniformly steady* in the experiment. Professor Osborne Reynolds has shown, however, that if the plate be given a *sharp push* and *then released*, this vortex is formed and *remains stable*. It appears to be purely a question of allowing the vortex to move along *at its own pace*. In the ordinary way, when the plate is being accelerated, the vortex falls to the rear a little; another vortex appears to be formed in front of it, and the first vortex is destroyed. These broken vortices then form what are called "eddies." This vortex is of the same type as those formed when "smoke-rings" are made—a form of spherical vortex. I have dealt, at some considerable length, on the formation of these vortices as well as of smoke-rings in the "A.B.C. of Hydrodynamics," showing how they are formed as well as how they moved. The explanation of the formation of smoke-rings was, however, purely theoretical, for I quite failed in all my attempts at photographing them *whilst being formed*. In the September and October numbers of the *Journal of the Franklin Institute* (1911), I see that Mr. Edwin F. Northrup has successfully accomplished this. He gives some very beautiful photographs of rings (formed in water) at the very earliest stages of formation, and these agree remarkably closely with theory, as well as with Avanzini's Fig. 5.

* Whether Avanzini ever proposed this formula, or not, I am unable to say. I have never seen the paper, nor have I ever come across anybody who had; but in any case it does not agree with the results of the experiments which he undoubtedly published and which I have seen and studied.

In Fig. 6 we see the form of the vortex when the angle of attack is acute—all that has been said about the vortex in Fig. 5 applies equally to this one; but this latter has some special peculiarities of its own. The centre of the vortex is not opposite the centre of figure—the vortex is not symmetrical. The strength of that part of the vortex which is nearest to n^1 is not equal to that of the part near l^1. If the vortex were free—as it would be in the case of a "smoke-ring" vortex—the half nearest l^1 would travel faster than the part near n^1, and it would eventually be *in front* of this half, instead of in rear. The half near n^1 would thus be the stronger and would overtake the then leading half. The vortex would consequently oscillate as smoke-rings are well known to do. The centre of pressure on the posterior side of the plate would therefore shift about and oscillate through the centre of figure of the lamina. Increasing the velocity of the moving plate shifts this centre of pressure towards n^1 thus reducing the pressure on l^1o^1 and so causing the angle of attack to be reduced, as stated in Law II.

NOTES

The "Akron" Disaster. The "Akron" dirigible, which was built for the purpose of crossing the Atlantic, was totally destroyed by an explosion, at about 6.30 a.m. on July 2, while manœuvring 1,500 feet above the sea at Atlantic City. Melvin Vaniman and his crew of four men, including his brother Calvin, were killed. Commenting on this distressing tragedy, our esteemed contemporary, the *Scientific American*, says: Vaniman, like many an inventor before him, died a victim to his own theories; and his sudden taking-off is rendered the more pitiful by the fact that his theory was correct, and had he only waited for the completion of his new "wire-cloth" machine, he might have escaped disaster. In seeking to construct a dirigible of constant displacement, with a shell of sufficient strength to withstand the increase of pressure due to rise of temperature, he was treading the one path—at least so we judge—along which development of the dirigible must proceed, if it is to become a really practical machine. . . . Vaniman has left behind him in his wire-cloth fabric a material which may yet prove to be the solution of the hitherto unsolved problem. The failure of his weaker rubber-cloth fabric to stand the tangential stress proves only that in this particular case it was unequal to its duty. The wire-cloth is over 30 times stronger.

Aeroplane Fatalities. The following list of fatalities brings up the total of deaths for 1912 to 66. It is a long list and a sad one, the outstanding feature being the large proportion of passengers killed. Among the better-known names, one notices Philip Parmelee, the famous Wright pilot, the first to carry goods—i.e., a consignment of silk bales—on an aeroplane (7 Nov., 1910). He was killed on a tractor biplane of his own design which refused to flatten out after a steep dive—a very common fault of this type, due to a great extent, probably, to insufficient surface in the tail elevator. Albert Kimmerling was one of the pioneer pilots; he was the first man to fly in South Africa (December 28, 1909). Miss Harriet Quimby was the first woman to gain a pilot's licence in America, and also the first woman to fly the Channel (April 16, 1912). Captain Loraine was in the front rank of British military flyers, and Staff-Sergeant Wilson was one of the first two pilot N.C.O's. in the Regular Army.

With regard to Miss Quimby's accident, E. L. Ovington, who examined the wreck, states, according to the *Aero and Hydro* (Chicago), that he found one of the duplicate left-hand rudder control wires of the Blériot had caught over the lower end of the warping lever. "Of course," he adds, "this is a defect in construction, as the rudder wires should

either have been put further away from this warping lever, or else been run through sleeves at this point so as to prevent them becoming entangled with it." The reason why the accident had not happened before was because a bar lever had been used in this machine instead of the customary *cloche*.

Date. 1912	Name.	Nationality.	Machine.	Place.
May 5 ..	Mdme. Galanschki	Russian	—— (2)	Riga
May 5 ..	Olbrechts	Belgian	—— (2)	St. Job (Antwerp)
May 10 ..	Lieut. Depersis	Italian	Farman (2)	Pordenone
May 11 ..	Schmigulski	Polish	——	Cassel
May 13 ..	E. V. B. Fisher V. L. Mason (P.)	English American	Flanders (1)	Brooklands
May 14 ..	Capt. Echeman	French	Blériot (1)	Etampes
May 25 ..	Lieut. von Schlichting (P.)	German	Fokker (1)	Johannistal
June 1 ..	E. Réby (P.)	French	Bréguet (2)	Juvisy
June 1 ..	P. Parmelee	American	Avro-type (2)	Seattle
June 2 ..	H. Buckstätter Lieut. Stille (P.)	German	——	Bremen
June 6 ..	G. Rost	German	——	Fuhlsbüttel
June 9 ..	A. Kimmerling M. Tonnet (P.)	French	Sommer (1)	Mourmelon
June 9 ..	H. Levasseur (P.)	Belgian	Hanriot (1)	Bétheny
June 11 ..	A. L. Welsh Lieut. Hazelhurst (P.)	American	Wright (2)	College Park (Maryland)
June 17 ..	Mrs. Julia Clark (*née* Irving)	English	Curtiss (2)	Springfield (Ill.)
June 19* ..	Capt. Dubois	French	Bréguet (2)	Douai
June 19* ..	Lieut. Peignan	French	Bréguet (2)	Douai
June 22 ..	H. Turner	American	Curtiss type (2)	Hempstead (L.I.)
June 22 ..	Lieut. F. von Falkenhayn	German	Aviatik (1)	Döberitz
June 26† ..	Lieut. Etienne (P.)	French	——	Near Guyancourt
June 29 ..	Schadt	German	—— (1)	Mulhausen
June 29‡ ..	Capt. C. Bayo	Spanish	Farman (2)	Madrid
July 1§ ..	B. Konig	German	—— (1)	Hamburg
July 2 ..	Miss H. Quimby W. Willard (P.)	American	Blériot (1)	Boston (Mass.)
July 3 ..	Lieut. Caranda	Roumanian	Farman (2)	Bukharest
July 5 ..	Capt. E. B. Loraine Staff-Sergeant R. H. V. Wilson (P.)	English	Nieuport (1)	Salisbury Plain
July 9 ..	R. Bedel	French	Morane (1)	Chalons
July 11 ..	V. M. Smith	American	——	Palo Alto (Cal.)
July 15 ..	G. Olivérès	French	Sommer (2)	Bourg
July 18 ..	Lieut. Preusser	German	Grade (1)	m. Leipsic
July 27 ..	J. Fischer H. Kugler (P)	German	Otto (2)	Munich

* Met in collision.

† Date of death. Accident, June 10.

‡ Date of death. Accident, June 27.

§ Date of death. Accident, June 30.

(P.)=Passenger.

(1) =Monoplane.

(2) =Biplane.

NEW BOOKS AND PUBLICATIONS

LUFTSCHRAUBEN. LEIT FADEN FÜR DEN BAU UND DIE BEHANDLUNG VON PROPELLERN. Paul Béjeuhr. Frankfurt: F. B. Auffarth, 1912. Illus. pp. 180. 4 marks.

*BAU UND BETRIEB VON PRALL-LUFTSCHIFFEN. R. Basenach. Frankfurt: F. B. Auffarth, 1912. Illus. pp. 101. 3 marks 50.

ALL THE WORLD'S AIRCRAFT (AEROPLANES AND DIRIGIBLES). Fred. T. Jane. (3rd Annual Issue.) London: Sampson Low, 1912. Illus. pp. 406. 21s. n.

*A.B.C. OF HYDRODYNAMICS. Lieut.-Col. R. de Villamil, R.E. (ret.). London: E. & F. N. Spon, 1912. Illus. pp. 135. 5s. n.

THE AEROPLANE IN WAR. C. Grahame-White and H. Harper. London: Werner Laurie, 1912 Illus. pp. 244. 12s. 6d. n.

*THE FLIGHT OF BIRDS. F. W. Headley, M.B.O.U. London: Witherby, 1912. Illus. pp. 163. 5s. n.

THE DYNAMICS OF MECHANICAL FLIGHT. Sir George Greenhill, F.R.S. London: Constable, 1912. Illus. pp. 117. 8s. 6d. n.

[These lectures, which were delivered at the Imperial College of Science and Technology, March, 1910 and 1911, will be the subject of a full review in our next issue.]

PROPOSITIONS DE M. V. BJERKNES. Christiania: A. W. Broggers, 1912. (C.I.A.S. Publns.)Diags. pp. 8.

DIE MÜNCHENER REGISTRIERBALLONFAHRTEN IM JAHRE 1911. August Schmauss. München: A. Buchholz. 1911. pp. 41.

MÜNCHENER PILOTBALLONBEOBACHTUNGEN IM JANRE 1911. J. Höllerer and K. Bux. München: E. Mühlthaler, 1912. pp. 20.

*PRACTICAL GYROSTATIC BALANCING. Herbert Chatley, B.Sc. London: Technical Publishing Co. 1912. pp. 50. Illus. 2s. n.

VOLAMEKUM: HANDBUCH FÜR LUFTFAHRER. A. Vorreiter and H. Boykow. München: Lehmann, 1912. Illus. pp. 168. 4 marks.

REPORT OF THE ADVISORY COMMITTEE FOR AERONAUTICS FOR THE YEAR 1911-12. London: H. M. Stationery Office, 1912. pp. 27. 2d.

A BRIEF ACCOUNT OF THE AËRONAUTICAL SOCIETY. London: Aëronautical Society, 1912. Illus. pp. 52. 1s. n.

* Reviewed in this issue.

REVIEWS

Practical Gyrostatic Balancing. By Professor Chatley, B.Sc. London: Technical Publishing Company, Ltd, 1912. pp. 50. Illus. 2s. n.

THIS book by Professor Chatley may certainly be considered one of the best from this author's pen, in fact among such ones as are known to the reviewer, it *is* the best. The subject is an admittedly difficult one, and to a still greater degree, is a confusing one, the motions being apparently so unnatural, but Professor Chatley has managed to propound the principles in a surprisingly lucid manner, tabulating where necessary, and giving examples, the better to help the understanding.

Starting with a reference to the characteristic properties of spinning tops, the author proceeds to the consideration of the mechanical principles involved, and the relations between Torque, Precession, and Spin; this leads at once to possibilities of Gyrostatic Control, and introduces such considerations as the necessity of, and conditions for "slow precession." Then follows by far the most fascinating of all Gyrostatic Problems, viz., the righting of a Gyrostat by "forcing the precession" and its beautiful application to monorail vehicles by Louis Brennan. This is well explained (except for a small point on page 42) on pp. 39—43.

Relay mechanisms are next discussed, and applications to ship control and flying machines follow. These are dealt with in a general way, rather than by particular descriptions, in fact there is no descrpition of an actual apparatus—an omission to be regretted. For obtaining a concise and lucid insight into the principles which must be employed when designing or considering any method of Gyrostatic Control, this little book will be found most useful.

T. W. K. C.

The Flight of Birds. F. W. Headley, M.B.O.U. London: Witherby, 1912. Illus. pp. 163. 5s. n.

THIS little book will be found interesting by aeronauts as it gives not only general information about Bird Flight, but also a mass of detailed particulars which are not usually found in either ornithological or aeronautical treatises.

In Chapter I. "Gliding" is described, its general principles explained, and the necessity for perpetual forward movement over fresh air pointed out. The author is of opinion that the kestrel does not hover in still air, and that a fair breeze which so to speak renews the air upon which his wings are working, is necessary to enable him to perform this manœuvre. As regards the curvature of the wings he points out that very fast flyers such as swifts have very little concavity and that speaking generally the amount of curvature of a wing is inversely proportional to the speed of flight. Various other points are discussed, such as the area of the supporting surface, the lift and drift, &c. The general principles of "Stability" are described in Chapter II., and an account is given of certain details which make for "automatic stability." Particular attention is drawn to the part played by the elastic rear edges of the wings in controlling and regulating the stability. This is a very good chapter, the notes on "Voluntary Adjustments" being particularly interesting.

In Chapter III. "Motive Power" is discussed and the different phases of the wing stroke in flapping flight explained.

Chapter IV. contains a very good account of "Starting" and the difficulties in connection with this act. As regards Helmholtz's theory (or practically speaking, Froude's "Law of Comparison") the author's remarks are, however, hardly fair. One and perhaps the principal reason why there are no large birds is that while the cross-sectional area of a muscle increases say as l^2, the tensional strength must increase as l. Now as far as is known at present there is no difference between the tensional strengths per sq. unit of the muscular material of large and small birds, and consequently the conditions required by the Law of Comparison are not fulfilled.

In Chapter V. the author discusses "Steering," and among other matters points out one which has hitherto received very little attention, viz., the flexibility of the backbones of birds, and shows that as far as his measurements go, good steerers have supple waists, while in poor ones the waists are comparatively speaking stiff and awkward to move rapidly. "Stopping and Alighting" are explained in Chapter VI. and the "General Machinery of Flight" in Chapter VII. This latter subject is gone into in considerable detail, and the descriptions given will be found very useful. It would be an advantage if a good diagram showing exactly how the pectoral muscles act were given, as their action is as a rule very little understood.

Chapter VIII. is taken up with "Varieties of Wings and Flight," and some interesting information is given. As regards the speed and height of flight the details are, practically speaking, the same as those described in the FIRST REPORT OF THE AËRONAUTICAL SOCIETY'S BIRD CONSTRUCTION COMMITTEE. The author draws attention to the fact that at great heights the rarefaction of the air obviously assists the flight of migrant birds, but he appears to consider favourable breezes to be the most important factor.

As regards lasting power and endurance attention is drawn to the fact that the normal temperature of birds is very high, viz., up to 111° F. in some species, and that this points to a very high vital power.

The connection between "Wind and Flight" is discussed in Chapter X., and the different motions when flight is performed in disturbed air explained.

The book is very well illustrated.

J. D. FULLERTON, COL. R.E. (RET.).

A B C of Dynamics. By Lieut.-Col. R. de Villamil, R.E. (ret.). London: Spon, 1912. Illustrated. pp. 135. 2s. 6d. net.

For the student of aerodynamics there is no better preliminary study than hydrodynamics, and Col. R. de Villamil's book should prove valuable to those who wish to acquire a good general knowledge of the subject. The point aimed at in the volume is to obtain an accurate idea of what actually takes place when solid bodies move in fluids or vice versa. The author is never dull and does not overload one with mathematics, but calls to his aid the opinions of leading professors who have contributed to the science, Lord Kelvin (to whose memory the volume is dedicated), Professor Osborne Reynolds,

Professor Penny, Dr. Froude, Stokes and Lanchester are freely quoted, and in cases where there is a disagreement the author does his best to clear the way, and in this he is generally influenced by the reasoning of Lord Kelvin.

Perhaps the most interesting chapters are V. to IX. dealing with viscosity; the author in his discussion uses Dr. Froude's well-known experiments with long flat boards towed edgewise to prove that the resistance due to viscosity varies as the *shearing surface* and as the velocity only, viscosity is considered to be a stress set up in the liquid to resist shearing; at a "critical speed" the liquid breaks up into eddies and the resistance caused by these eddies may be considered to vary as V^2, the formula for the total resistance being $AV+BV^2$, A and B being constants. The author will have nothing to do with empirical rules, which within limits are known to give sufficiently accurate results. For this reason the book will appeal more to the scientific student than the engineer.

Col. de Villamil gives us very little idea of the practical value of some of the scientific teaching he is anxious to impart, nor does he always tell us who first stated or demonstrated that teaching. For instance, from the author's description, one might almost conclude that "the important law connecting the pressure in a fluid with its velocity" was first demonstrated by Lord Rayleigh or Dr. Froude. It is surely interesting for the student to know that the law connecting the pressure in a liquid with its velocity was first thought of by the Italian Bernouilli about the year 1740, and Venturi, a fellow-countryman, verified Bernouilli's statement by experiment at the Philosophical Theatre of Modena about 120 years later. Further, an interesting point connecting theory with practice is that the application of this particular theory has enabled Herschell, an American engineer, to construct a remarkably accurate meter for the measurement of water in pipes.

The reader will find interesting chapters dealing with Helmholtz rings and vortex motion, and his subject generally so bears on the neighbouring science of aerodynamics that its perusal cannot prove other than instructive.

J. R. P.

Bau und Betrieb von Prall-Luftschiffen. By R. Basenach, Frankfurt-on-Main. Auffarth, 1912. Part I. Illus. pp. 101. 3 marks.

In this handbook the aim of the author is to give a clear and easily understood exposition of the essential points in the construction and working of non-rigid airships. The present volume is the general presentation of fundamental principles and designs. In two volumes to follow the special problems arising in construction and working will be handled with detailed description of actual current designs. It is quite probable, therefore, that this first volume may appeal to a different set of persons from that which will await with interest the two subsequent volumes.

One can readily recommend this handbook to educated laymen who desire to read a careful and comprehensive elucidation of the main considerations affecting the design and running of non-rigid airships but who do not care for the mass of detail which practical constructors might welcome.

The main points first discussed are: classification based on structure; principal points in design to ensure best attainable safety in working, high speed, and useful load with high soaring capacity.

Then follows a closer consideration of secondary points dependent on the conception of the purpose for which the airship may be destined. The desiderata in military airships are distinguished from the more modest demands of passenger carriers: Speed and the effect of wind, measurement of speed; lifting power, useful load and the influence of atmospheric conditions; height range and duration of journey; radius of action.

A special section is devoted to gas, in which not only are the physical and technical properties of hydrogen, &c. carefully discussed but commercial points are also treated: e.g., it is pointed out that in transporting H compressed in steel flasks a ten-ton load yields only 82 kilos., so that one has to pay in tare weight one hundred and twenty times the nett weight in H.

The author, in his chapter on design, after deducing in logical order the main problems to solve, points out that practice founded on pure theoretical data has failed, and therefore the appeal is more or less to empirical experience, this, naturally, is considered under: (1) design based on experience gained from models, and (2) design affected by the teaching of experience in actual working.

Two or three pages are devoted to the working out of an example for the preliminary design of a medium size airship. Attention is also given to current practice in deciding the weights of the main parts.

The last chapter deals with the problems of the airship in flight; air resistance, pressures, and kindred points.

Readers will probably agree with the author's remark, p. 17, "that [*passenger*] airships will probably never become common and as far as can at present be foreseen will be used solely for the conveyance of a relatively trifling number of passengers." But "military airships are destined to be used for strategical and tactical reconnoitring, fighting hostile airships, destroying and upsetting lines of retreat, destroying railway bridges, depôts, stations, docks, &c., and their lesser uses are in attacks on forts, camps, bivouacs, ships and crowded masses of troops." This conception of the destiny of airships amply justifies the perseverance of Germany in the construction of dirigibles. J. E. H.

The Mechanics of the Aeroplane. By Captain Duchêne. Translated by J. H. Ledeboer and T. O'B. Hubbard. London: Longmans, Green, 1912. pp. 231. Illus. 7s. 6d. n.

THIS is an excellent translation of a work which should be in the library of every student of aeronautics, not only because it groups together the basic truths of aeroplane theory in such a way as to save much labour of reference, but because these truths are expressed so simply and clearly. The book is divided into four parts under the respective headings:—"Flight in still air, Equilibrium and Stability of the Aeroplane in still air, Effect of Wind on the Aeroplane, and Propulsion." The treatment under these heads is qualitative mainly, such numerical calculations as are employed being approximate only.

In Chapter I. is explained the action of a wind-current striking a plane. It is interesting to see that nil angle of incidence of a curved plane is taken to be the angle at which no lift occurs. This sound practice now finds many adherents. In Chapter II. horizontal flight in still air is dealt with very clearly. Captain Duchêne makes very good use of graphs of the various characteristics of the aeroplane, similar to those which are a feature of other French text-books, and arrives at some very neat formulæ, whence are derived four practical rules, applicable to an average aeroplane of to-day, for calculating the speed, thrust, and motive power necessary for horizontal flight in still air. An excellent chapter.

"Oblique flight in still air" is equally well treated in Chapter III., which closes Part I. Captain Duchêne, unlike M. Alexandre Sée, considers that the claims of stability make it imperative to choose the highest of the two possible speeds at which a given aeroplane can fly.

In considering equilibrium and stability in straight flight (Chapter IV.), a popular error is exposed which likens the lateral stability of an aeroplane to that of a boat.

The centre of pressure next receives attention, and a defect is noticeable here which is exemplified elsewhere in the book, in that Captain Duchêne refers to certain facts as if they had recently been discovered by his compatriots, when in reality they were brought to light long ago in other countries. For example, the fact that at low angles of attack the suction at the back of a plane is responsible for practically all the lift was demonstrated in Denmark by Messrs. Vogt and Irminger about 1895. It is doubtless quite by inadvertence that Captain Duchêne's reference to the fact appears to give the credit for this demonstration to M. Eiffel, but the point is worthy of mention now that so much history is being made that early workers are likely to be overlooked by the student. Again, M. Soreau appears to receive the credit for explaining the bird's utilisation of gusty winds, Langley not being mentioned.

Longitudinal, lateral and directional stability are considered in detail and some interesting results arrived at. It is shown that lowering the centre of gravity has not the large effect on the stability of an aeroplane that is usually attributed to it. Captain Duchêne considers that when aeroplanes can fly at small angles of incidence (smaller than at present) an inverted dihedral to the wings may present advantages. In Chapter V. (Turning), much stress is properly laid on the correct proportioning of the keelplane for purposes of stability. In considering the action of the wind on the aeroplane (Part III., Chapter VI.) the effect of the fictitious "regular" wind is first examined, and its modification of the flight path, range of action, &c., explained. The action of the usual irregular wind is then considered, and the conclusion arrived at that the machine of the future should fly at high speeds to combat such winds.

Captain Duchêne believes that eventually machines will be made quite stable automatically and considers that "feelers" (such as the plane of the Doutre stabiliser) are more promising as disturbance detectors than gyroscopes. He does not, however, give his reasons for this belief. He very soundly states that until the stability problem is solved aeroplanes can have only a limited field. Chapter VII. deals with Propulsion and is devoted to a very clear exposition of the theory of the screw propeller. Captain Duchêne keeps an open mind, as evinced by a note recognising that other means of propulsion than the screw may be employed in future. On page 202, line 6, "rational" appears instead of "rotational."

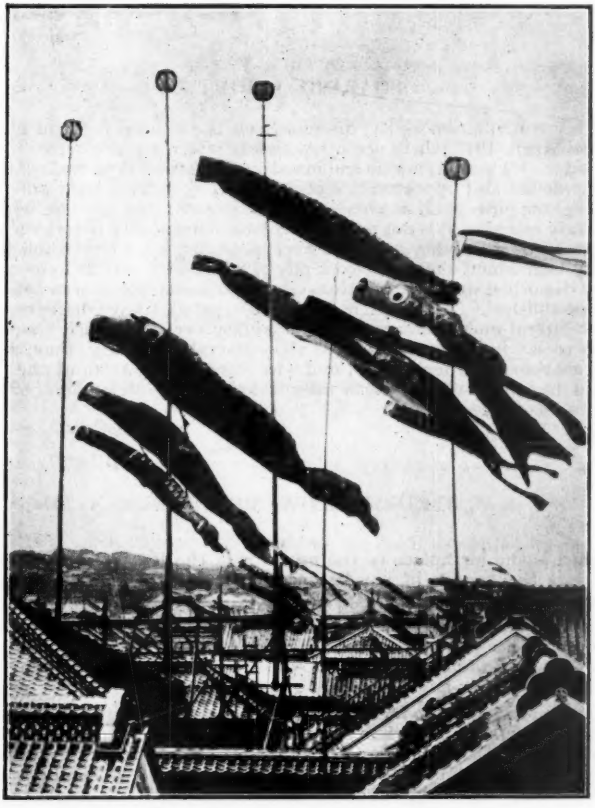
Some useful tables of square, conversion tables and so forth finish off a most excellent text-book. B. G. C.

CORRESPONDENCE

To the Editor of the AËRONAUTICAL JOURNAL

AN AIR TARGET FOR ARTILLERY

SIR,—In response to my invitation to designers and others, to produce an air target which could be employed for the aerial practice of artillery (see January number of this JOURNAL, page 11), Mr. Alan Owston, a member of the Society residing at Yokohama, has forwarded a picture post-card showing a number of "koi," or carp, made of some light fabric, attached to masts, and flying in the breeze; also one of the actual carp, about 8 feet in length. The mouth of the fish is stitched to a circular hoop, the tail is open at the ends; a bridle is fixed to the hoop at the mouth, and attached to a tall mast, and the wind passing through the carp's body keeps it inflated and flying at an angle whose incidence varies with the strength of the wind; these fish are flown on festive occasions.



Mr. Owston's idea is that one of these carp might be towed by an aeroplane, being attached thereto by a piano wire, which would run off a small reel; he suggests that "failing some better device for handling this target, it should be folded up like a flag, and 'broken out' when the aeroplane was in full flight."

The idea seems well worth following up. The first thing to do is to get one of our pilots to experiment with the fish at the end of a thousand yards of towing wire; the wire would be wound on a reel, and at the end of the run would presumably have to be

cut loose, so as to avoid any danger of complication in turning and landing. The initial experiments could of course be made with a short length of wire, merely with a view to ascertaining the best method of fixing up the reel and attaching the wire, so as to avoid risk of interference with any of the controls. Obviously an aeroplane for this purpose should be driven by a tractor screw. Such experiments could only be carried out over unoccupied land, or over an area subject to the necessary safety precautions. If the experiments were successful, the next step could be considered, viz. : how to fire at the target without endangering the pilot of the aeroplane. This could be managed under certain conditions, but would not be a very simple matter ; the precautions would have to be of such a nature as to make it impossible to shoot the pilot or the aeroplane by mistake. It is not necessary to enter into the nature of these precautions now ; the question is to prove the feasibility or otherwise of towing the target at the end of a thousand yards of towing wire.

Perhaps some of the readers of the JOURNAL will be sufficiently interested to try the experiment with the fish, which can be lent for the purpose. If the experiment is successful on a small scale, it would be desirable to repeat it on a larger scale with a fish at least 30 feet in length.

F. G. STONE, COL., R.A.

SOARING FLIGHT

SIR,—In my contribution to the discussion on the soaring flight of birds, published in your issue of April, 1912, there are a few points which require correction in the light of later knowledge. I stated that lateral instability only occurs in birds of heavy loading. With further practice and opportunities of observation, I have been able to see lateral instability in lighter birds such as cheels and scavengers. On days on which instability occurs in the early morning, it is not unusual for lateral instability to be seen first, followed by dorso-ventral axis instability. Then after soarability has been established transverse axis instability begins and the first two kinds of soarability are no longer observed. It is only in very disturbed weather that transverse axis instability is seen before widespread soarability is established. Although, as I had anticipated, I have discovered exceptional cases in which lateral and transverse axis instability occur together, the rule still holds good. This is to say in air in which birds show lateral instability, they, as a rule, show no signs of transverse axis instability, and vice versa. The facts of the case are complicated, and I have met with various unexpected results, an account of which I hope to publish on a latter occasion.

E. H. HANKIN.

STRINGFELLOW MEMORIAL

July, 1912.

SIR,—Chard is the birthplace of the inventor of the English Aeroplane, upon John Stringfellow rests the honour of having been the first man to make an engine-driven aeroplane, which flew in the year 1848.

Being a friend of the inventor, and having seen the original machine in its working shed, I have taken it upon myself to issue this circular for the purpose of soliciting contributions to enable a tribute to be erected to the memory of one who rendered such great service to his country and to science.

This tribute would take the form of an Imperishable Bronze Wreath, to be appended to the obelisk of the family memorial to Stringfellow in the cemetery at Chard with appropriate inscription. The stone curbing surrounding the grave would have a neat palisading surmounting it with aeroplanes in relief upon it. A tablet will also be erected over the porch of Stringfellow's late home.

The amount of money required to enable the foregoing proposals to be carried out is not a large one, and is estimated at about fifty pounds (£50).

I am, yours faithfully,

JAMES GILLINGHAM.

Prospect House,
Chard, Somerset.